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The Price and Allocation Effects of Targeted Mandates: Evidence from Lead Hazards

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Abstract

Several states require owners to mitigate lead hazards in old houses with children present. I estimate the mandates' effects on housing markets. My empirical strategy exploits differences by state, year, and housing vintage. The mandates decrease the prices of old houses by 7.1 percent, acting as a large tax on owners. Moreover, families with children become 11.3 percent less likely to live in old houses. Increases in rents for family-friendly houses suggest that the mandates have important distributional consequences. These findings are relevant for evaluating similar mandates such as healthy homes standards.

KEYWORDS: Mandates; Health Hazards; Housing Quality. JEL CODES: I18, Q52, R21.

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1 Introduction

Lead poisoning is associated with reduced IQ (Ferrie et al. 2015) and educational attainment (Reyes 2015*b*, Aizer et al. 2018) and an increased risk of criminal activity (Reyes 2007, 2015*a*, Feigenbaum & Muller 2016, Grönqvist et al. 2020, Aizer & Currie 2019).[1] Lead paint was extensively used in the first half of the last century, until a growing recognition of these lead hazards motivated its ban for residential purposes in 1978. The Department of Housing Development (HUD, 2011) estimates that nationwide, lead paint lingers in 5.5 million houses inhabited by small children, the population most at risk for lead poisoning, resulting in lead hazards in 21 percent of houses with small children (Dewalt et al. 2015).[2] Beginning in 1971, an increasing number of states mandated abatement, i.e., control of lead hazards in older houses. Yet, abatement is expensive: Koppel & Koppel (1994) estimate that it can cost between \$500 and \$40,000, depending on the extent of the lead hazard, and funding for abatement is limited. Therefore, the mandates are analogous to a group-specific tax on buyers of old houses (Gruber 1994).

This paper presents the first large-scale evidence on the effects of state abatement mandates on the housing market. I compare outcomes for new and old houses, which are more likely to have lead hazards, within a state before and after a mandate's introduction, in a difference-in-differences framework that exploits identifying variation at the state-year-vintage level. My empirical analysis proceeds in two steps: first, I focus on property values to assess the incidence of the mandates on property owners; then, I analyze how the mandates affect which homes different households live in, that is their allocation across houses. To estimate the effect of the mandates on house values, I use sales data, collected by DataQuick from public deeds. To assess how households' allocation changes with the mandates, I use data from the American Housing Survey (AHS). I examine multi- and single-family homes separately as a proxy for rental and owner-occupied homes, respectively.[3]

[1] This figure refers to children with blood lead levels (BLLs) above $5\mu g/dL$. In 1991, the CDC defined BLLs$\geq$ $10\mu g/dL$ as the level of concern for children aged 1–5 years. Since 2012, the term "level of concern" has been replaced with an upper reference interval value defined as the 97.5^{th} percentile of BLLs in US children aged 1–5 years from two consecutive cycles of National Health and Nutrition Examination Survey (NHANES), currently at $5\mu g/dL$.

[2] Since the deleading of gasoline between 1973 and 1995, lead paint in houses built before the 1978 ban is the major source of lead exposure in the United States.

[3] In this paper I refer to dwellings as homes or houses. In the analysis, the transaction data are at the property level,

The hedonic model for differentiated goods guides my empirical analysis (Rosen 1974). Prior to the mandate, owners abate homes with low abatement costs, and households sort into safe and hazardous homes based on their willingness to pay. The mandate requires owners to further abate old houses in the presence of small children, even where it is not profitable. Thus, the mandate imposes an expected tax, decreasing property values. Abated properties recoup value over time and attract families with children as they are made lead-safe, while the value of non-remediated properties stays low.[4] Therefore, long-term effects of the mandates can shed light on compliance rates in the absence of abatement data.

The transaction data show that the costs imposed by the mandates are reflected into lower house prices, although the mandates do not affect sales. Both old multi- and old single-family houses fall in value by 7.1 percent for as long as ten years. These large reductions in prices can be rationalized noting that even if a home does not present an immediate lead hazard, costly maintenance practices are needed to avoid future hazards and that reduction in prices may reduce further investments. The allocation data show that prior to the mandates, high-income families with small children disproportionally live in new houses, consistent with avoidance of lead hazards. For a few years after a mandate, families with small children are 11.3 percent less likely to live in old houses than before. Together, several robustness checks support the validity and interpretation of these findings.

The decrease in both the value of old houses and the likelihood that families with small children live in these old houses after a mandate is consistent with low rates of abatement. Under a low abatement scenario, the mandates may decrease the probability that families with small children live in old houses through both demand- and supply-side channels. On the demand side, the mandate may spread novel information on the riskiness of certain homes, steering families with small children away from old houses. This inward shift in the demand for old houses by families with small children, might decrease rents for old homes, depending on the relative elasticity of supply and demand. On the supply side, owners of old homes could charge higher rents to fami-

while in the AHS each unit constitutes an observation.

[4] In the paper, I use the term families to refer to households.

lies with small children via price discrimination in segmented markets, or might avoid renting to these households.[5] Price discrimination allows landlords to capture part of the difference in utility households attain in a leaded home vs. in a, still more expensive, unleaded home. I examine the effects of the mandates on rental prices for family-friendly homes to gather suggestive evidence in favor of supply- or demand-side mechanisms. Consistent with supply-side adjustments dominating, rents for old family-friendly houses appear to increase by 3.1-8.5 percent after a mandate.

These findings suggest that assessing the impact of the abatement mandates requires characterizing changes in the market allocation, in line with the literature on the unintended consequences of mandates (Summers 1989, Gruber 1994, MaCurdy & McIntyre 2001). As households sort into homes and neighborhoods based on their utility and budget constraint (Tracey & Walsh 2008, Kahn 2000), they implicitly select their pollution exposure, which affects long-term outcomes (Currie et al. 2011, Cohen-Cole 2006, Evans 2006, Gazze 2016). Banzhaf et al. (2019) discuss how these demand side forces, as well as supply-side considerations and political economy factors may result in higher exposure levels for households of low socioeconomic status. In the context of lead poisoning, the principal-agent problem inherent in the landlord-tenant relation may lead to lower investments in remediation and even discrimination (Davis 2011*b*). By analyzing the effects of the mandates on housing values, housing allocation, and rental prices, I shed light on how these mechanisms interact. Moreover, Banzhaf et al. (2019) highlight how the ecological fallacy may lead to measurement issues: because old houses coexist in the same neighborhood with newer homes, my granular data is key to correctly measure differential exposure risk. The estimates in this paper can be extrapolated to evaluate similar policy proposals, such as changes in housing standards and requirements for healthy homes.

This paper contributes to a growing literature studying lead poisoning prevention policies. First, Aizer et al. (2018) show that Rhode Island's abatement mandate for all rental properties successfully decreased lead poisoning. My state-by-state analysis shows that Rhode Island is the

[5] Audit and correspondence studies find that when looking for housing, minority households are shown homes in areas with lower levels of environmental amenities (Christensen & Timmins 2018, Christensen et al. 2020). Anecdotal evidence suggests that some landlords might discriminate against families with small children to avoid incurring these abatement costs (Berman et al. 2013, Williams 2010).

only state where the mandate increased property values, suggesting that the state's lead-safe certificate model is particularly effective at spurring abatement. Second, Billings & Schnepel (2017) find that federally-funded lead remediations increase home values and have tremendous returns on investments. By showing that unfunded remediation mandates decrease property values and fail to attract families with children into old homes, my findings suggest that liquidity constraints might prevent some landlords from undertaking beneficial investments. Third, Bae (2012) and Bae (2016) find that the federal mandate to disclose information about lead hazards to potential buyers and renters appears to increase buyers' testing and reduce purchases of old homes among families with small children, but has seemingly no effect on the value of old houses. My finding that abatement mandates decrease the value of old houses may be due to abatement being perceived as more costly than disclosure or to methodological differences in the studies. By analyzing state mandates that are implemented at different points in time, I can control for localized time trends.

The paper proceeds as follows. Section 2 discusses the potential effects of an abatement mandate in the context of a hedonic model. Section 3 provides background on the mandates I study and describes the data I use. Section 4 estimates the impact of the mandates on house prices, the allocation of households across houses, and rents. Section 5 concludes with policy implications.

2 Theoretical Framework

This section characterizes the housing market responses to the introduction of an abatement mandate in the context of the hedonic model for differentiated goods (Rosen 1974). Like an environmental standard, a mandate effectively rations the availability of leaded homes, improving the average quality of old homes. Unlike other environmental cleanups, lead abatement mandates are unfundend and group-specific, that is they cover houses with children. Because cleanup costs fall on property owners, the mandates may reduce property values. The incidence of group-specific mandated benefits depends on the group's valuation of the benefit (Gruber 1994). Thus, a mandate may decrease the share of families with small children living in old houses through either supply-side responses, including price discrimination or refusal to rent to families with small children, or demand-side responses. Supply-side responses imply increases in rental prices for families with small children, while demand-side responses may lead to lower rental prices.

I consider a closed housing market where renters differ along two dimensions: income and whether they have children. Homes are in fixed supply and differ in terms of the housing services they provide, for example size and age. In addition, old houses might have lead paint, while new homes are lead-free. While households without children do not care about lead safety, the willingness to pay for lead safety of households with children depends on their income. Owners of old homes can voluntarily abate lead hazards at heterogeneous costs that depend on owners' characteristics, such as credit access, or home characteristics.

Absent a mandate, a fraction of old homes is abated such that the markets for leaded and lead-safe homes clear, and the rent for leaded homes is weakly lower than the rent for lead-safe homes other things equal. Holding other characteristics constant, families without children prefer the cheaper leaded homes. Families with children sort into lead-free homes based on their indirect utility, which depends on income. By revealed preferences, remaining leaded homes have abatement costs that are higher than the present value of the rent premium for a safe home.

2.1 The Mandate Equilibrium

Unexpectedly, the government introduces an unfunded mandate requiring leaded houses to be abated conditional on occupancy by a small child. I discuss the effects of the mandate on property values, rents, and sorting of households into leaded and lead-safe homes. Because I do not observe whether old homes are lead-safe in the data, I consider effects on the average old home.

Property Values. This mandate is analogous to a tax on leaded properties that is levied on the buyer. Thus, the value of these properties falls by an amount equal to the difference between the abatement cost and the present value of the rent premium for a safe home, which is positive by revealed preferences. Hence, the average value of old homes declines, too.

Rents. Abated homes shift supply of leaded homes in and supply of lead-safe homes out.

Moving along the demand curve, rents for leaded homes increase relative to lead-safe ones. The effect on average rents in old homes is ambiguous, but rents in old homes increase *relative* to rents in new ones. As abatement is a long-term improvement, these changes should persist over time.

Allocation. As lead-safe homes become cheaper, marginal households with children move to lead-safe homes, including newly abated old homes. Thus, the share of high-income households with children in old homes should increase.

Summing up, this simple framework suggests that after a mandate, old properties sell at lower prices. Moreover, abatement increases the supply of lead-safe homes relative to leaded homes, increasing the relative rent for old homes and increasing the share of households with children in old, newly abated homes.

2.2 Information, Price Discrimination, and Housing Supply Elasticity

So far, I have assumed perfect information about the dangers and prevalence of lead hazards, perfect enforcement and uniform pricing, and fixed housing supply. Here, I discuss the implications of relaxing these assumptions.

Information. A mandate can provide an information shock if households are not fully informed about lead hazards in homes at baseline. For example, the media might cover the health effects of lead exposure and highlight which homes are more likely to have lead hazards. Such an information shock would decrease demand for leaded homes by families with small children, decreasing the share of households with children in old homes in the short run, until property owners abate some old homes and the market converges to a new equilibrium. As the mandate also shifts supply of leaded homes in, the net effects on rents depends on the relative elasticities of demand and supply. While it is difficult to disentangle the information content of the mandates empirically, I explore this issue in Section 4.1.2.

Price Discrimination. Even under imperfect enforcement, the mandates increase the marginal cost to rent a leaded house to a family with a small child because the child's presence might trigger abatement. If owners of old houses are able to price discriminate based on family status, they will charge families with small children a higher rent than families without children. In particular, low-income households may be less able to defend their tenants' rights. Some families will still choose leaded homes if their indirect utility is higher than the utility they would attain in the, still more expensive, unleaded home of their choice. This price discrimination would move the equilibrium allocation along the demand curve to a point with fewer families with children in old houses and higher rents for these families.

Summing up, on one hand the mandate may decrease rents in leaded homes by decreasing their demand through an information shock, provided supply is sufficiently inelastic relative to demand. On the other hand, the mandate may increase rents in leaded homes, and especially so in market segments targeted at families with children that enable price discrimination. Thus, changes in rents can help distinguish between changes in the housing allocation due to shifts in demand and those due to supply-side responses such as price discrimination. Section 4.3 explores how the mandates affect rental prices for old family-friendly homes.

New Construction. New constructions can replace old homes or expand housing supply. A mandate may push old homes at the upper tail of the distribution of abatement costs out of the market by making it more profitable to demolish them and build new ones. Thus, land availability and the elasticity of supply of new housing likely mediate the effects of the mandate: in areas with a more inelastic housing supply, the same shock will have a stronger effect on prices. Similarly, underlying population trends may magnify or attenuate the effects of the mandate. For example, in shrinking cities where exising homes are already in excess supply, falling prices of old homes following a mandate are less likely to spur substitute new construction, implying larger price declines for old homes than in growing areas (Glaeser & Gyourko 2005). Section 4.2 investigates how housing market characteristics mediate the mandates' effects.

3 Background and Data

3.1 Regulatory History of Lead Paint

The incidence of lead paint in the current housing stock increases with structures' age: HUD (2011) estimates that 87 percent of houses built before 1940 in the US have lead paint, compared to 69 percent for houses built between 1940 and 1959 and 24 percent for houses built between 1960 and 1977. Starting in the late 19th century, paint contained up to 50 percent lead by weight to increase its durability (Reissman et al. 2001). In the 1950s, the growing body of evidence of the harm associated with lead induced some manufacturers to voluntarily reduce the lead content of paint to 1 percent, a level that can still induce severe lead poisoning (Hammitt et al. 1999).[6] Finally, in June 1977, the Consumer Product Safety Commission lowered the allowed level of lead in paint to 0.06 percent, effectively banning lead paint for residential purposes. Still, the ban does not cover the pre-existing housing stock, although lead remains in a house indefinitely unless it is carefully removed (Mushak & Crocetti 1990).

When paint surfaces deteriorate, residents, and especially children, are exposed to health hazards from lead-contaminated dust. Lead dust enters the human system through ingestion or inhalation. Small children are especially exposed to lead-contaminated dust from paint and windowsills due to normal hand-to-mouth activity (Fee 1990). Moreover, lead is most damaging to small children: they absorb and retain more lead than adults and their neurological development is particularly susceptible to neurotoxins (see, e.g., McCabe 1979).

As of today, 19 states have enacted mandates that require abatement of lead hazards on interior and exterior surfaces.[7] For the empirical analysis, it is crucial that the timing of the mandates is uncorrelated with unobservable housing market trends that could confound the estimated impact

[6] Meanwhile, the use of lead paints for interior decoration also decreased. Therefore, houses built after 1950 are more likely to only have lead paint on exterior surfaces. Exterior lead paint can still constitute a hazard for children because lead dust deposits in the soil outside the home and can be ingested when children play outside.

[7] City governments may also enact regulations. While these local initiatives likely cause heterogeneity in treatment effects, I lack systematic data to study these differences more in depth. To the extent that the timing of these city-level regulations is not correlated with the introduction of the state-level mandates, the absence of local data does not affect the validity of my estimates of average treatment effects.

of the regulations. Table 1 shows that states introduced lead abatement mandates in three waves.[8] Early adopters such as Massachusetts and New Jersey started regulating lead abatement in the 1970s, when lead paint made the national agenda. A second wave of states, such as Connecticut, introduced mandates in the 1990s, when new medical evidence led the CDC to lower the poisoning threshold to 10 $\mu g/dL$. Finally, more states such as Rhode Island, Ohio, and Michigan introduced abatement mandates in the 2000s, when the CDC started requiring states receiving funding for lead poisoning prevention activities to "develop and implement strategic childhood lead poisoning elimination plans" (CDC, 2005). In the empirical analysis, I formally assess the validity of the identifying assumption of parallel trends. Moreover, Appendix Figure C.1 shows no evidence of a correlation between the timing of the mandates and trends in construction, population, or GDP.

These mandates differ in terms of their coverage, what triggers a lead order, and type of abatement required (Table 1). If the owner fails to abate, residents have several protection measures to enforce their right to abatement, from rent withholding to lawsuit in housing courts that can result in high fines.[9] In the main analysis, I estimate the average treatment effect of these mandates by pooling all states together to exploit the exogeneity of the timing of the mandates across states.

3.2 Data

I combine two data sources to analyze the impact of the mandates on house prices, housing allocation, and rents.

Housing Prices. To assess the impact of the mandates on home values, I analyze price data at the transaction level obtained from the DataQuick data repository.[10] This is a dataset of public records of property sales (e.g., price, date, mortgage type) from 1988 until 2012 and of property characteristics collected from the most recent publicly available tax assessment and deeds records from municipalities across the US. The assessor file includes details on the physical characteristics

[8] Regulations were identified with a search through LexisNexis and Westlaw.

[9] In addition, partial funding in the form of loans and grants might be available for low-income property owners wishing to abate lead hazards, although most resources are restricted to homeowners.

[10] I accessed the data repository, housed at the Taubman Center for State and Local Government at the Harvard Kennedy School, during a visiting period under the Exchange Scholar Program.

(e.g., square footage, number of bathrooms, number of stories, year built), use type (e.g., residential, commercial, single-family, condominium, tenancy), and street address for every property in the covered counties. I choose this data source for two reasons. First, the granularity of these data allows me to control for census tract fixed effects that restrict the comparison of outcomes across houses in the same neighborhood. Second, sales data provide a more precise estimate of the value of a property than assessed values or survey data; however, if the mandates affect the rate at which old houses are transacted, the estimates of mandates' effect on prices will suffer from selection bias. Appendix Tables C.1 and C.2 show that while the mandates affect the probability of sale of multi-family homes, houses transacted before and after the mandate appear to be similar. Thus, selection bias does not appear to be a concern in this context.

The assessor data cover approximately 90 percent of housing structures nationwide, although different counties enter the sample in different years, (Appendix Figure B.1). Six implementing states are covered both before and after they introduce a mandate, namely, Connecticut, Georgia, Michigan, North Carolina, Ohio, and Rhode Island (Table 1). The 2.5 million transactions in these states provide the identifying variation for the empirical analysis, while the other implementing states help estimate trends. Connecticut, Michigan, Ohio, and Rhode Island have enough pre-mandate observations to assess the validity of the parallel trends assumption, with a total of 2 million transactions. My main findings rely on the differential timing of the mandates across states for identification, and are robust to using a more balanced panel and to dropping any one of these states, as discussed below. Moreover, coverage appears to be only mildly correlated with measures of the state regulatory context used in the literature (Gyourko & Molloy 2014), assuaging the concern that transaction data are only available for states with more stringent lead laws.

I estimate the effect of the mandates on house prices separately for rentals and owner-occupied properties. In the assessor file, I infer that a house is owner-occupied if the owner's mailing address is the same as the property address. However, tenancy decisions are likely endogenous. Hence, my preferred specification splits the sample across single- and multi-family homes, a fixed characteristic, and I interpret the findings for multi-family homes as proxies for rentals.[11]

Allocation and Rents. To analyze the impact of the mandates on occupancy and rents, I use the AHS National Sample, years 1985-2011. The AHS is a biennial panel of housing units, i.e., surveyors visit the same houses in each wave, and it includes a vast array of property characteristics, including binned construction year, as well as household demographics and tenure duration. Unfortunately, the public use data only provide identifiers for Metropolitan Statistical Areas (MSAs) and not state identifiers. Since the mandates are state-level policies, I drop observations in MSAs that cross state borders, resulting in 211,994 observations in 36 states. Column 4 of Table 1 reports which implementing states are in the AHS sample. Among those states, the ones that implement a mandate after 1985 provide the identifying source of variation for the empirical analysis.

Comparison of the two datasets. Appendix Table B.1 compares the characteristics of the housing stock in the DataQuick and the AHS data sets. These samples appear similar in terms of the size of the housing units, as well as house values and age of the housing stock.

4 Empirical Analysis

I estimate the effect of the mandates on housing markets by comparing outcomes for old and new houses within a state before and after a mandate, stacking observations across states. This setting yields a difference-in-differences (DD) design where old houses comprise the treated group. The identifying variation for this analysis is at the state-year-vintage level for states that implement a mandate during the sample period. In the hedonic model, the value of a house is a log-linear function of housing characteristics. Thus, I estimate the effect of the abatement mandates on prices and allocation by fitting the following equation:

$$Y_{ivst} = \beta Mandate_{st} * Old_{vs} + \pi \mathbf{X_{it}} + \gamma_{sv} + \delta_{tv} + \eta_{st} + \varepsilon_{ivst} \quad (1)$$

[11] Appendix Table C.3 shows that my main findings hold when splitting the sample based on tenancy.

where Y_{ivst} is an outcome for house i of vintage v, in state s and year t, $Mandate_{st}$ is an indicator for year t being the year of the mandate's introduction in state s or any year thereafter, Old_{vs} is an indicator for houses targeted by the mandate, $\mathbf{X_{it}}$ is a vector of potentially time-varying house characteristics and local amenities, and δ_{tv}, γ_{sv}, and η_{st} are year-vintage, state-vintage, and state-year fixed effects respectively. Specifically, Old_{vs} equals one for houses built before 1978 in every state but Maryland where the mandate targets only houses built before 1950. Vintage refers to century of construction for houses built in the 1700s and 1800s and to decade for the 1900s.[12]

The controls included in $\mathbf{X_{it}}$ vary depending on the sample. One one hand, the panel nature of the AHS sample allows me to control for unit fixed effects, improving the precision of my estimates.[13] On the other hand, the richness of the transaction sample allows me to include tract-year and tract-vintage fixed effects that replace the respective state-level interactions. The introduction of tract fixed effects restricts the analysis to the comparison of old and new houses within a small area with a population of less than 10,000 individuals.[14] In addition, tract-vintage fixed effects control for local variation in the characteristics of the housing stock built at different times. For instance, local availibility of natural gas determines the heating fuel of houses built at a given point in time (Davis & Kilian 2011, Myers 2019).[15]

By including state-year or tract-year fixed effects, I control non-parametrically for state-specific or tract-specific trends in the housing market that may be correlated with the introduction of the mandates. Correlation would arise, for instance, if changes in amenities, such as urban flight and urban decay, which are associated with decreasing house values, lead to poorly maintained houses, and hence higher lead hazards and a stronger push to enact preventative regulations. Alternatively, a correlation could arise if salience of lead paint hazards in old homes increases with the state-level phase-out of leaded gasoline. The setback of this specification is that I cannot estimate the effect

[12] In the AHS sample I drop houses built after 2000, as they only appear in less than half of the panel years.

[13] In alternative specifications, I include fixed effects for number of units, stories and rooms in the property.

[14] There is considerable variation in the age of the housing stock even within such small areas: in my sample, half of the tracts have between 31 and 89 percent of houses built before 1978.

[15] The prevalence of gas-heating vs. oil-heating is mostly constant in the 1970s and the early 1980s, with no evidence of a discontinuity around the year 1978 (Appendix Figure C.2). My results are robust to focusing on houses built in a small window of years around 1978, confirming that my findings are not driven by spurious fluctuations in fuel prices (Appendix Table C.4).

of the mandates on the *level* of prices, i.e., the potential spillovers of the policies on new houses.

The internal validity of the DD framework hinges on the assumption that old and new houses are on parallel trends prior to the mandates, i.e. the timing of the mandates is uncorrelated with the error term conditional on the control variables. This would be violated, for instance, if the mandates were systematically accompanied by revitalization programs targeted at old houses. Section 3.1 discusses how the timing of the mandates appears to follow advancements in the medical knowledge around lead exposure. To verify the plausibility of the parallel trends assumption, I estimate a year-by-year version of the DD, as in the following equation, and present plots of the leads, α_y, and lags, β_y, of the mandates' effect on old houses:

$$Y_{ivst} = \sum_{y=1}^{T_{min}} \alpha_y Pre_{t-y,s} * Old_v + \sum_{y=0}^{T_{max}} \beta_y Post_{t+y,s} * Old_v + \pi \mathbf{X_{it}} + \gamma_{sv} + \delta_{tv} + \eta_{st} + \varepsilon_{ivst} \tag{2}$$

This section analyzes the effect of the mandates on sale prices (Section 4.1) and the housing market allocation (Section 4.2). Then, Section 4.3 relates the change in house values and allocation to the effect of the mandates on rents for houses with different characteristics to provide suggestive evidence on the mechanisms driving the observed housing market allocation.

4.1 Sale Prices

I estimate the effect of the mandates on sale prices in the DataQuick sample separately for the rental and owner-occupied market as proxied by multi- and single-family homes, respectively. By revealed preferences, an abatement mandate should reduce the value of old homes as owners had opted not to abate absent the mandate (Section 2). I use the natural logarithm of price per square foot as my preferred outcome as remediation costs are generally proportional to square footage. This is equivalent to using log price on the left-hand side and controlling for log square footage on the right-hand side. My estimates are robust to using log price on the left-hand side and controlling for a quadratic polynomial of square footage (Appendix Table C.5).

Figure 1 plots year-by-year DD estimates from a version of equation (2) that controls for tract-year fixed effects: abatement mandates erode the value of older homes relative to newer ones, both for multi-family (left panel) and single-family houses (right panel). In both panels, the relative price of old houses is fairly constant up to several years prior to the mandate, and it falls significantly after the mandate. Table 2 presents the corresponding point estimates for old multi- and single-family houses: after a mandate, both types of houses fall in value by 7.1 percent on average (Column 1), a result that is not driven by pre-existing trends (Column 2) and is robust to controlling for house characteristics or house fixed effects (Columns 3-4).[16] Consistent with lead hazards being more prevalent in houses built in the first half of the 20^{th} century, Figure 2 shows that the effect of the mandate is stronger for older vintages.

Figure 1 shows a persistent depreciation of old houses after the introduction of a mandate, a finding that is not consistent with high abatement rates in the average treated state. This persistent effect may be in part due to building and demolition patterns in each neighborhood, as houses built in the 1990s appear to increase in value relative to houses built in the 1980s after the mandates (Figure 2).[17] Appendix Table C.8 examines heterogeneous effects across states which might be related to heterogeneity in the mandates' implementation, focusing on four states with data available for several years both before and after the regulations, Connecticut, Michigan, Ohio, and Rhode Island. Importantly, these regressions do not control for vintage-level trends, which are absorbed in the regressions that pool all states together. Rhode Island exhibits a unique pattern: old properties appear to increase in value for six years after the mandate and then decrease after 2008. Rhode Island stands out also for high screening rates, 80 percent for three-year-old children, and the related high compliance rates with the mandate: the total number of lead-safe certificates issued to landlords increased from 333 to 47,734 between 1997 and 2010 (Aizer et al. 2018). Unfortunately, there is little reliable data on inspections and remediations in other states to establish a direct link between enforcement and remediation rates in each state and housing prices. Yet, Ohio sees a spike in the number of licensed inspectors in 2006 and a ramp up in the number of licensed contractors

[16] Appendix Table C.6 shows qualitatively similar results using self-reported housing values for owner-occupied homes in the AHS data, the data used in Bae (2016).

[17] The relative point estimates are shown in Appendix Table C.7.

starting in the same year, three years after the mandate (Appendix Figure A.4). One can speculate that uncertainties in the implementation of these mandates could explain why the mandates' effects are small in Michigan and even positive in Ohio for the first years after the mandates' introduction.

I estimate large losses in house values following the mandates: prices of old multi-family houses drop by $5.49 per square foot on average, in the same order of magnitude as abatement costs, implying a high perceived probability of abatement and high pass-through rates to sellers, especially considering that not all pre1978 homes have lead hazards.[18][19] Billings & Schnepel (2017) find that federally-funded lead remediations increase home values in Charlotte, NC, by $20,000, with a 179 percent return on investment. Both our estimates are puzzling under rational expectations, even when one considers the high costs associated with lead poisoning lawsuits.[20] However, the observed average abatement cost is an underestimate of the true abatement cost, for at least two reasons. On one hand, we only observe abatement costs conditional on abatement, meaning that observed costs belong to the lowest tail of the cost distribution. On the other hand, the observed abatement cost does not take into account funding costs, psychic costs of interacting with government bureaucracy, or the opportunity costs of rent missed during abatement. Moreover, the mandates might foster maintenance and costly avoidance behavior, imposing a liability on these homes even when they do not get abated.

4.1.1 Alternative Explanations and Robustness Checks

Older houses transacted up to three years after a mandate lose 2.4 percent relative to newer homes in their census tract, and the loss in value is over 10 percent in later years (Table 2: Column 2). This lagged effect is surprising: owners should immediately internalize the costs induced by the

[18]Nationwide data on HUD-funded remediations for the period 2001-2018 indicate that these remediation projects cost $7,250 on average. Pre-1978 homes in my sample measure 1,566 square feet on average, implying an average abatement cost of $4.64 per square foot. I consider this an underestimate of abatement costs since HUD grants are only available to low-income households who might choose cheaper remediation projects.

[19]These estimates are in line with the literature on the capitalization of pollution and school investments (Currie et al. 2015, Greenstone & Gallagher 2008, Gamper-Rabindran & Timmins 2013, Davis 2011*a*, Muehlenbachs et al. 2015, Bartik et al. 2019, Cellini et al. 2010).

[20]The media report settlements for these lawsuits in the millions. However, no systematic data exists from housing courts on the number of lawsuits initiated against landlords.

mandate. Three potential explanations can help rationalize this lagged effect. First, low baseline child lead screening rates could explain delayed compliance and enforcement. As more children are screened over time, the mandates gain bite and the probability that an owner has to abate increases. Second, federal requirements concerning abatement and renovation work on old homes became more stringent over time, thus increasing costs. For example, in 2008 the Environmental Protection Agency passed regulations requiring contractors working on pre1978 homes to acquire certification of lead safe practices. Third, the mandates may induce lower investments in maintenance and renovations in older homes in anticipation of lower resale values, which contribute to increasingly lower home values over time.[21] For example after a mandate, owners of old homes might find it less profitable to invest in child-friendly amenities or house expansions.

While I cannot disentangle the role of these potential explanations of the lagged and persistent mandate effects, I can rule out that two potential confounding factors drive this lagged effect: 1) the unbalanced nature of my data panel and 2) filtering. First, a balanced sample including all untreated states covered since 1988 and only those treated states with price data for at least one year prior to seven years after the introduction of a mandate yields very similar estimates to those plotted in Figure 1 (Appendix Figure C.3). Second, homes might depreciate over time as the housing stock expands through a down-filtering process in a way that is not captured by the vintage-year fixed effects in my specification (Lowry 1960, Rosenthal 2014). As placebo mandates in untreated states have no effects on prices (Appendix Figure C.4), I reject that filtering is the main driver of the patterns in Figure 1.

More broadly, within-city trends such as the decline of inner city neighborhoods, differential effects of the 2008 housing crisis, or gentrification might confound my findings. The tract-year fixed effects and the staggered adoption of the mandates across states ensure that local trends are not the main drivers of the results. Moreover, focusing on a pre-2008 sample yields similar patterns to those in Figure 1 (Appendix Figure C.5), showing that my findings are not driven by stronger effects of the 2008 housing crisis on older homes.

[21] See Billings (2015) for an example of how increased property values to due light rail transit improvements led to increased renovation activity in Charlotte, NC.

A further concern in interpreting the effect of the mandates on prices is that the mandates might change the composition of houses that are transacted. For example, the price decline observed for old houses could be due to an increase in the supply of lower quality old houses. While the mandates affect the probability of sale of multi-family homes, houses transacted before and after the mandate appear to be similar (Appendix Tables C.1 and C.2). Therefore, selection does not appear to be driving the results in Table 2.

Finally, the results in Panel A of Table 2 are generally robust to different samples and specifications, such as allowing for state-vintage specific linear trends (Appendix Tables C.5 and C.9).[22] Together, these robustness checks support the validity and interpretation of my findings even if some present weak estimates.

4.1.2 The Role of Information

In this section, I investigate the likelihood that the mandate acts as an information shock by exploring heterogeneous effects based on the likelihood that the buyer has a child and the buyer's information about lead hazards.

Families with small children or planning to have small children, may both be more aware of the mandates and their costs and perceive a higher likelihood of enforcement. Then, neighborhoods with more families with small children would see the largest drops in the price of old houses. Census tracts that had a higher share of children in 1980 indeed see larger decreases in the value of old homes after the mandates (Table 3). Specifically, old houses in tracts in the first quartile of the distribution of children residents depreciate by 4%. These tracts have on average 4.7 children over 100 residents. Doubling the share of children to 9.2 in the third quartile almost doubles the negative effect of the mandate on old houses by an additional 3.2 percentage points. However, this relationship does not appear to be linear at the highest tail of the distribution, which cautions from interpreting the share of children in a census tract as the mere probability of enforcement.

[22]Limiting the sample to implementing states only reduces the estimated price effects. This is likely because the fixed effects for vintage by transaction year estimated on this smaller sample differ, potentially indicating that idiosyncratic variation within these states gets too much weight in the smaller sample.

To investigate the role of awareness of lead hazards and of the mandates' requirements further, I hypothesize that buyers from states where lead paint is more prevalent and states that eventually introduce mandates are more likely to know about lead hazards and abatement. I determine a buyer's residence at the time of the purchase by matching the buyer's name to a seller's name in another transaction in the DataQuick sample within a three year window through a fuzzy match.[23] I obtain a linked sample of 2.84 million observations. By construction, the matching algorithm excludes first-time home-owners, who may be more likely to start a family. The sample selection may explain why the mandate's effects estimated on this sample (Table 4) are an order of magnitude smaller than the effects in the full sample, which caveats the results from this exercise. Nonetheless, Table 4 suggests that the negative effects of the mandates on old houses are driven primarily by transactions in which the buyer comes from a high-knowledge state. Thus, I conclude that the effect of the mandates on the value of old homes cannot be explained solely by the mandates providing new information about lead hazards and their prevalence. The mandates' effects on rents, discussed in Section 4.3 below, further support this interpretation.

4.2 Allocation

By affecting prices and housing quality, the mandates may also change the housing allocation and the distribution of exposure risk. Prior to the mandates, high-income families with small children are less likely to live in old houses than other households (Figure 3). After the mandates, fewer low- and middle-income families with small children live in old homes.[24] To establish causality, I compare household characteristics, my outcome variables, in old and new houses before and after a mandate by estimating equation (1) with unit fixed effects in the AHS sample.

After a mandate, 11.3 percent fewer families with small children live in old houses after (Table 5).[25] In contrast, people over 59 years of age are no less likely to live in old houses, and if anything,

[23] I restrict the match by blocking on first and last initials.

[24] Appendix Table C.10 describes the average households in old and new houses before and after mandate.

[25] In each AHS wave, on average over 50 percent of households with small children move: my estimates are compatible with some of these households moving to new houses rather than old ones. Turnover in multi-family units appears to increase the first years after a mandate, but this effect fades 4 years after the mandate (Appendix Table

they replace families with small children.[26] The probability that a small child lives in an old house after a mandate decreases even more for families with income below the median, by 24 percent.[27] These results do not support the hypothesis that the mandates spur abatement, and suggest that supply-side responses like price discrimination against families with small children or demand-side reactions to increased salience of lead hazards might mediate the effects of the mandates, as discussed in Section 2.2. While Section 4.1.2 suggests that the effect of the mandates on the value of old homes cannot be explained solely by an information story, Section 4.3 further attempts to disentangle supply- and demand-side mechanisms by examining the mandates' effects on rents.

Plotting period-by-period estimates from equation (2), Figure 4 shows that the effect of the mandates on allocation fades after five years. Increasing abatement over time cannot explain families with small children returning to old homes, as it would also increase housing values in contrast with the persistent depreciation observed in Figure 1. A potential explanation is that the mandates temporarily increase the salience of lead hazards: salience induces families with small children to sort out of old homes exerting downward pressure on rents, yet buyers of old properties continue to factor in future abatement costs independently of their family status and reduce investments in upkeep of these old homes. Related, Billings & Schnepel (2017) find that owners in Charlotte, NC, appear to invest more in renovations after they abate lead hazards.

A second potential explanation for the temporary allocation effects is that, as mandates decrease the price of old houses, home-owners with small children are more willing to accept the risk of lead hazards, moving along the demand curve. Then, markets with a more inelastic housing supply, where price effects are larger, should also see smaller adjustments on the allocation margin, as discussed in Section 2.2. Using data on the elasticity of housing supply from Saiz (2010) and tract-level population counts from the Census, Table 6 shows suggestive evidence in favor of this hypothesis. It is important to note, however, that the AHS sample may be underpowered for this analysis, especially as most MSAs in the sample show positive population growth at the time of

C.11).

[26] Appendix Table C.12 shows that the results in this section are robust to different specifications.

[27] Accordingly, Appendix Table C.13 shows that the mandates affect occupancy in multi-family houses the most.

the mandate's introduction.

4.3 Rents

Sections 4.1 and 4.2 show that the mandates lower the value of old houses and that fewer families with small children live in old houses after a mandate. What explains this sorting pattern? Section 2.2 discusses how an information shock would lower demand for old houses by families with small children, potentially resulting in lower rents for old houses depending on the relative elasticity of demand and supply. Alternatively, price discrimination may lead to higher rents for families with small children living in old houses. To disentangle these mechanisms, I estimate equation (1) in the AHS sample controlling for unit fixed effects.

First, Table 7 fails to reject the null hypothesis that the mandates increase rents for old houses overall. This finding can be due to the fact that new owners purchased homes at lower prices, and therefore do not need to pass on abatement costs. This null result can also mask important heterogeneity. For example, market segments in which supply of rental homes is more elastic could see disproportionate exit of old homes after a mandate, which in turn might exert upward pressure on rental prices. Appendix Table C.14 shows suggestive but imprecise evidence that the mandates reduce the number of old single-family rentals, leading to a temporary increase in rents for these units, but not for those in multi-family homes.[28]

Second, I identify homes that are attractive to families with small children based on fixed characteristics, such as the number of bedrooms or the presence of a small child at baseline (year 1985). Table 7 shows suggestive evidence that rents for family-friendly homes increase after a mandate, albeit the standard errors are quite large and estimates change signs in Columns 5-6. Specifically, rents for old family-friendly homes appear to increase by between 3.1 and 8.5 percent, or $234-642 yearly (Columns 4 and 7, the most stringent specifications). These rent increases in the family-friendly market segment are more consistent with supply-side price adjustments limited to family-friendly houses than with demand-side information shocks, as discussed in Section 2.2.

[28] In my sample, 57% of multi-family properties are not the owner's primary residence.

5 Conclusion

This paper estimates the impact of lead abatement mandates on the housing market in a DD framework. I exploit the state-level variation in the timing of the mandates, as well as the regulations' focus on old houses and families with small children, to investigate 1) the mandates' incidence, and 2) the mandates' effect on children's risk of lead exposure. First, owners of old properties face a large cost, as the mandates decrease the value of old houses by $5.49 per square foot. Second, the mandates appear to have ambiguous effects on children's exposure risk. On one hand, fewer families with small children live in old homes up to five years after a mandate. On the other hand, the persistent depreciation of old homes suggests that abatement rates are low on average and that those children remaining in old homes may still be exposed to lead hazards. Importantly, I lack comprehensive data to evaluate the welfare effects of the mandates, including estimates of moving costs and actual abatement rates.

The targeted nature of the mandates aims to address the main issue with lead hazards in US homes: as families with small children represent a small fraction of the population, it is neither cost-effective nor feasible to require abatement of the entire US housing stock at once. Suggestive evidence of increases in rents for old family-friendly homes implies that family with small children may also bear part of the mandates' costs. A flat rental registration fee for old properties such as the Rhode Island system studied by Aizer et al. (2018), could improve efficiency if the revenues from the fee were used to subsidize abatement in the presence of small children. Indeed, the Rhode Island mandate alone appears to increase the value of old houses in the state, suggesting that it might be effective at inducing remedation. These findings are helpful in thinking about policy proposals to regulate housing standards and healthy homes.

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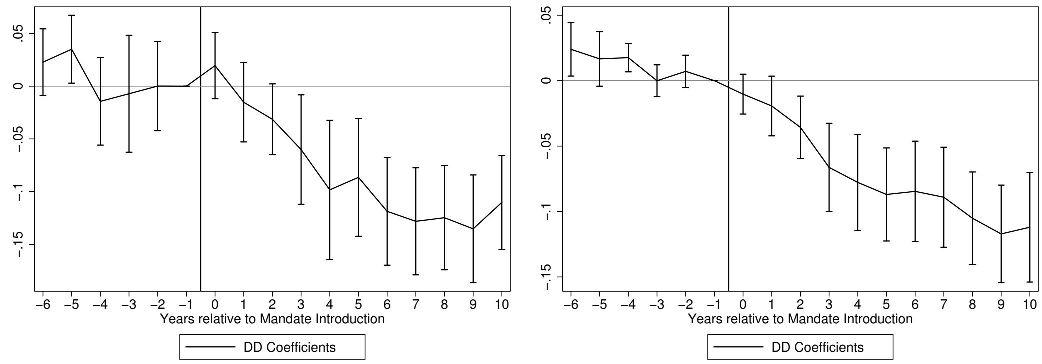
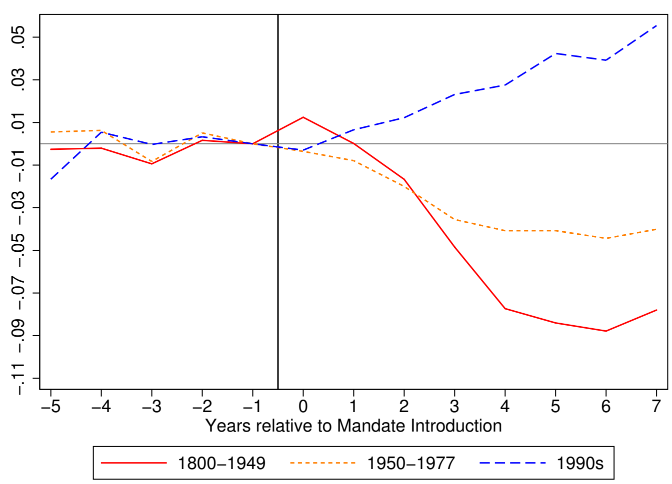
Figure 1: Price Effects



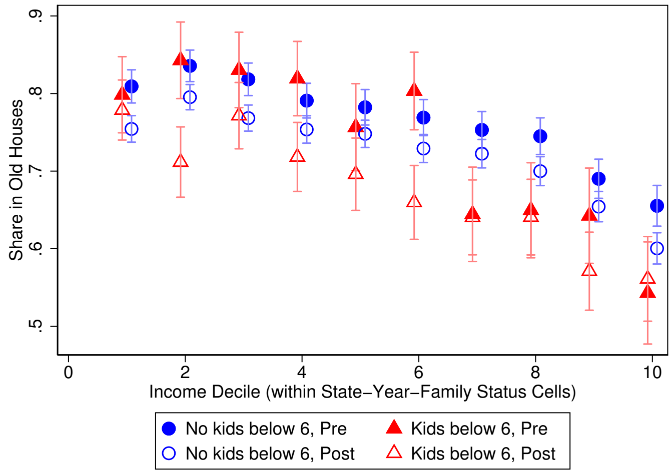
The figure plots DD coefficients on year-by-year mandate dummies, estimated on the DataQuick samples (1988-2012) of multi- (left panel) and single-family (right panel) houses. Each census tract is weighted by 1980 population. The outcome variable is the logarithm of the price per square foot. The vertical line indicates the introduction of the mandate. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Tract-year, tract-vintage and vintage-year fixed effects are included. T-1 is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level (42 clusters).

Figure 2: Price Effects, By Year of Construction



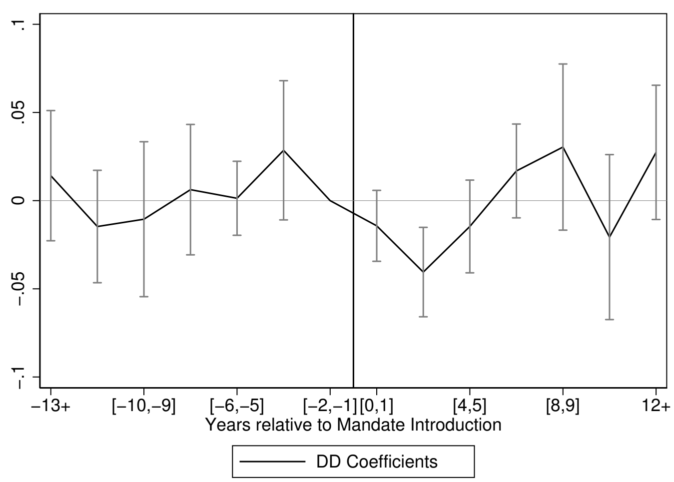
Notes: The figure plots DD coefficients on year-by-year indicators for homes built 1800-1949, 1950-1977, and in the 1990s relative to homes built 1978-1989, estimated on the DataQuick sample (1988-2012). Each census tract is weighted by 1980 population. The outcome variable is the logarithm of the price per square foot. The vertical line indicates the introduction of the mandate. T-1 is the omitted category. Tract-year, tract-vintage and old-year FE are included.

Figure 3: Sorting into Old Houses, By Income and Family Status



Notes: The figure plots the share of families in the AHS sample with (red triangles) and without (blue dots) children living in old houses in implementing states before (solid) and after (empty) the introduction of the mandates, by income decile. The vertical bars are 95 percent confidence intervals. The sample is limited to houses built between 1950 and 1999.

Figure 4: Allocation Effects: Child Under Six



The figure plots DD coefficients on two-year mandate dummies, estimated on the AHS sample (1985-2011). The outcome variable is a dummy for the household having a child below six years of age. State-year, year-vintage, month of interview and unit fixed effects are included. The vertical line indicates the introduction of the mandate. $T \in [-2,-1]$ is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level (36 clusters).

Table 1: State-Level Abatement Mandates

State (1)	Enactment Year (2)	DataQuick Start Year (3)	In AHS (4)	Rentals Only (5)	Trigger (6)	Coverage (7)
CT	1992	1988	Yes	No	<6	All
DC	1983	-	No	No	<8	All
GA	2000	1998 (1 county)	Yes	Yes	<6 with EBLL	Multifamily >12 units
IL	1992	1996	Yes	No	Child	All
KY	1974	2004	Yes	No	Child	All
LA	1988	2012	Yes	No	<6	All
MA	1971	1988	Yes	No	<6	All
MD	1995	1987	Yes	Yes	N/A	All
ME	1991	2005	No	No	<6	All
MI	2005	1991	Yes	Yes	N/A	All
MN	1991	1998	No	No	Child with EBLL	All
MO	1993	1998	No	No	<6	All
NC	1989	1988	Yes	No	<6 with EBLL	All
NH	1993	1996	No	Yes	<6 with EBLL	All
NJ	1971	1988	Yes	No	Child	All
OH	2003	1996	Yes	No	<6 with EBLL	All
RI	2002	1988	Yes	Yes	N/A	All
SC	1979	1990	Yes	No	Child	All
VT	1996	2002	No	Yes	N/A	All

The table displays the timeline of the introduction of abatement mandates in the 19 implementing states. Columns 2 and 3 contrast the mandates' enactment year with the year in which the state appears in the DataQuick Sample. Column 4 indicates whether the state is included in the AHS sample. Columns 5, 6, 7 characterize whether the mandate covers only rental homes, what triggers a lead order, and whether the type of buildings covered by the mandate.

TABLES

Table 2: Price Effects

Dependent Variable	Log Price per Square Foot				
	(1)	(2)	(3)	(4)	(5)
Panel A: Mandate Effects on Old Houses, Multi-Family Properties					
0-10 Years after Mandate	-0.071	-0.070			
	(0.017)	(0.016)			
2-6 Years before Mandate		0.002			
		(0.019)			
0-3 Years after Mandate			-0.024	-0.026	-0.028
			(0.014)	(0.013)	(0.023)
4-6 Years after Mandate			-0.100	-0.098	-0.116
			(0.028)	(0.026)	(0.037)
7-10 Years after Mandate			-0.122	-0.122	-0.125
			(0.018)	(0.017)	(0.020)
N	3,607,422	3,607,422	3,607,422	3,607,360	2,414,237
Price per SqFt, New Homes	105.763	107.718	105.763	105.763	105.897
Price per SqFt, Old Homes	77.373	80.886	77.373	77.373	81.333
Panel B: Mandate Effects on Old Houses, Single-Family Properties					
0-10 Years after Mandate	-0.071	-0.063			
	(0.013)	(0.013)			
2-6 Years before Mandate		0.011			
		(0.007)			
0-3 Years after Mandate			-0.043	-0.042	-0.043
			(0.013)	(0.014)	(0.015)
4-6 Years after Mandate			-0.089	-0.087	-0.091
			(0.018)	(0.019)	(0.023)
7-10 Years after Mandate			-0.109	-0.106	-0.093
			(0.017)	(0.017)	(0.018)
N	15,068,454	15,068,454	15,068,454	15,068,365	9,165,003
Price per SqFt, New Homes	108.628	109.845	108.628	108.628	111.593
Price per SqFt, Old Homes	102.204	111.003	102.204	102.204	104.132
Controls				X	
Property FE (Repeat Sales)					X

Notes: The table presents DD estimates on DataQuick samples (1988-2012) of multi- (Panel A) and single-family (Panel B) houses. Observations are weighted by 1980 population in tract. The outcome variable is the logarithm of the price per sqft. Tract-year, tract-vintage and vintage-year FEs are included. Column 4 controls for average room size and FEs for building condition, number of units, stories, and rooms in the building; Column 5 includes house FEs. For implementing states, the sample includes a $[-6, 10]$ window around the introduction of the mandates. Average price per sqft of new and old houses in implementing states before the mandates is shown. Standard errors clustered at the state level (42 clusters) are in parentheses.

Table 3: Price Effects, by Share of Children in Tract

Dependent Variable	Log Price per Square Foot	
	(1)	(2)
Mandate Effects on Old Houses	-0.057	-0.040
	(0.011)	(0.008)
Mandate Effects on Old Houses, High Share of Children	-0.017	
	(0.011)	
Mandate Effects on Old Houses, II Quartile Share of Children		-0.030
		(0.009)
Mandate Effects on Old Houses, III Quartile Share of Children		-0.032
		(0.010)
Mandate Effects on Old Houses, IV Quartile Share of Children		-0.038
		(0.020)
N	20,537,824	20,537,824

Notes: The table presents DD estimates on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. Quantiles in share of children in the population are determined at the tract level. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included. For implementing states, the sample is limited to a $[-6, 10]$ window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level are shown in parentheses.

Table 4: Price Effects By Buyer's Origin

Dependent Variable	Log Price per Square Foot				
	Full Sample	Buyer from Treated State		Buyer from High Lead Division	
		No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)
Mandate Effects on Old Houses					
0-3 Years after Mandate	0.004	0.007	0.000	0.010	0.002
	(0.011)	(0.007)	(0.009)	(0.009)	(0.010)
4-6 Years after Mandate	-0.008	0.013	-0.020	0.027	-0.019
	(0.013)	(0.014)	(0.009)	(0.022)	(0.010)
7-10 Years after Mandate	-0.020	-0.013	-0.021	0.008	-0.020
	(0.011)	(0.011)	(0.007)	(0.012)	(0.007)
N	2,837,279	2,008,521	654,338	1,422,723	1,236,834
Price per SqFt, New Homes	118.062	104.722	125.033	104.831	122.848
Price per SqFt, Old Homes	98.857	96.359	101.328	96.836	100.571

Notes: The table presents DD estimates on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The sample is limited to transactions for which the buyer could be linked to another transaction in which they were the seller. The outcome variable is the logarithm of the transaction price divided by square footage of the house. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Columns 1 and 2 split the sample according to whether the buyer sold a house in a treated state within 3 year of the transaction. Columns 3 and 4 split the sample according to whether the buyer sold a house in the Northeast, Middle Atlantic, East North Central, or South Atlantic within 3 year of the transaction. Tract-year, tract-vintage and vintage-year fixed effects are included. Average price per square foot in implementing states before the mandates is shown separately for new and old houses in each subsample at the bottom of each column. Standard errors clustered at the state level (42 clusters) ares shown in parentheses.

TABLES

Table 5: Allocation Effects

Dependent Variable	HH Has Child <6 (1)	HH Has Child <6, Poor (2)	HH Has Child 6-11 (3)	HH Has Child 6-11, Poor (4)	Youngest in HH >59 (5)	Youngest in HH >59, Poor (6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
After Mandate	-0.017	-0.012	-0.023	-0.011	0.026	0.023
	(0.008)	(0.006)	(0.017)	(0.005)	(0.019)	(0.011)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods						
0-3 Years after Mandate	-0.036	-0.021	0.001	0.000	0.009	0.019
	(0.011)	(0.007)	(0.018)	(0.004)	(0.019)	(0.012)
4-6 Years after Mandate	-0.020	-0.010	-0.036	-0.022)	0.028	0.013
	(0.012)	(0.008)	(0.021)	(0.008)	(0.019)	(0.013)
7-10 Years after Mandate	0.009	0.003	-0.038	-0.013	0.011	0.026
	(0.016)	(0.013)	(0.022)	(0.006)	(0.026)	(0.016)
10+ Years after Mandate	0.004	-0.009	-0.039	-0.012	0.080	0.046
	(0.014)	(0.010)	(0.030)	(0.009)	(0.023)	(0.011)
N	211,994	211,994	211,994	211,994	211,994	211,994
Outcome Mean, New Homes	0.162	0.028	0.155	0.022	0.199	0.077
Outcome Mean, Old Homes	0.150	0.051	0.153	0.049	0.266	0.110

Notes: The table presents DD estimates on the AHS sample for the years 1985-2011. Outcome variables are defined in each column. State-year, year-vintage, month of interview and unit fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

TABLES

Table 6: Mandate Effects, by Elasticity of Housing Supply

Market Characteristic	Housing Supply Elasticity				Population Growth			
Dependent Variable	Log Price per Square Foot		HH Has Child <6	Youngest in HH >59	Log Price per Square Foot		HH Has Child <6	Youngest in HH >59
Housing Type	Multi-Family	Single-Family	All	All	Multi-Family	Single-Family	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mandate Effects on Old Homes	-0.121	-0.099	-0.009	0.031	-0.094	-0.078	-0.236	-0.024
	(0.039)	(0.031)	(0.015)	(0.024)	(0.019)	(0.016)	(0.058)	(0.099)
Effects on Old Homes, High Elasticity	0.109	0.061	-0.026	-0.034				
	(0.039)	(0.030)	(0.021)	(0.030)				
Effects on Old Homes, Population Growth					0.040	0.012	0.222	0.051
					(0.008)	(0.007)	(0.064)	(0.094)
N	2,999,405	12,936,106	180,494	180,494	3,607,402	15,068,442	208,483	208,483
Mean Outcome, New Homes	103.975	107.091	0.162	0.194	105.763	108.628	0.162	0.199
Mean Outcome, Old Homes	85.293	100.774	0.151	0.266	77.374	102.204	0.150	0.266

Notes: The table presents DD estimates on the transaction sample from DataQuick (1988-2012) and allocation data from AHS (1985-2011) linked with housing supply elasticity data from Saiz (2010) and population growth data from the Census. Each observation in the DataQuick sample is weighted by tract population in 1980. The outcome variables are indicated in the second row. Columns 1,2,5,6 include tract-year, tract-vintage, and vintage-year fixed effects interacted with a high elasticity or positive population growth indicator; Columns 3,4,7,8 include state-year, year-vintage, month of interview, and unit fixed effects interacted with a high elasticity or positive population growth indicator. For implementing states, Columns 1,2,5,6 limit the sample to a $[-6, 10]$ window around the introduction of the mandates. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level are in parentheses.

TABLES

Table 7: Rent Effects by Number of Bedrooms and Children's Presence at Baseline

Dependent Variable: Log Monthly Rent	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post Mandate	-0.037	-0.041			-0.028		
	(0.024)	(0.025)			(0.026)		
Post Mandate, Old House	0.029	-0.022	0.001		0.018	0.064	
	(0.030)	(0.039)	(0.036)		(0.040)	(0.040)	
Post Mandate, 2+ Beds		0.007	-0.021				
		(0.032)	(0.032)				
Post Mandate, 2+ Beds, Old House		0.080	0.066	0.085			
		(0.037)	(0.033)	(0.016)			
Post Mandate, Child at Baseline					0.038	0.117	
					(0.237)	(0.246)	
Post Mandate, Child at Baseline, Old House					-0.032	-0.082	0.031
					(0.242)	(0.249)	(0.022)
N	83,432	83,430	83,430	83,028	65,761	65,761	65,431
Log Rent	5.851	5.750	5.750	5.750	5.705	5.705	5.705
Log Rent, Family-Friendly		5.901	5.901	5.901	5.901	5.901	5.901
Old Friendly = Old Not (p-val)		0.000	0.029		0.792	0.117	
Old Friendly = New Friendly (p-val)		0.089	0.093		0.954	0.947	
Property FE	X	X	X	X	X	X	X
YearXVintage FE	X	X	X		X	X	
StateXYear FE			X			X	
Family-FriendlyXYear FE			X	X		X	X
StateXYearXVintage FE				X			X

Notes: The table presents DD estimates on the AHS sample (1985-2011). Each column includes month of interview FEs. Mean outcome values in implementing states before the mandates are shown at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

TABLES

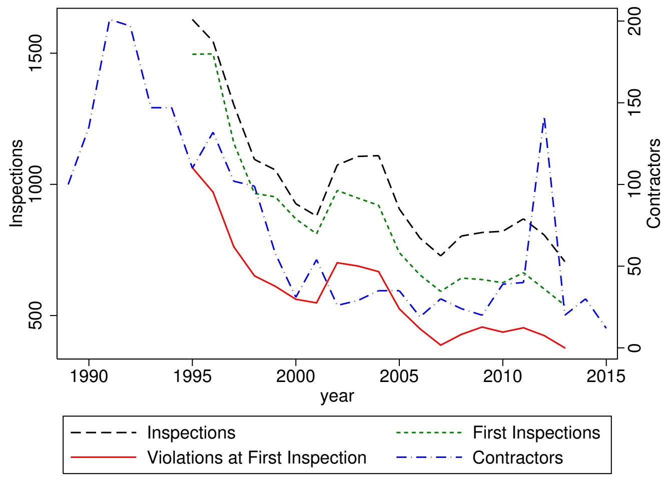
A The first stage on inspections and abatement

I collected data on lead inspectors and certified contractors, as well as on inspections and abatement projects, from selected states. The data are sparse and usually start after the introduction of the mandates, as the states set up registries in compliance with the regulations. Moreover, in general, voluntary inspections are not included. Finally, many of the inspectors' and contractors' licenses are dormant, as renewal costs are low compared to the initial fixed cost of obtaining a new license.

Here, I compare an early adopting state, Massachusetts, which introduced the lead mandate in 1971, with Ohio, which introduced it in 2004. Figure A.1 shows that, to this day, Massachusetts performs only 700 inspections per month, despite the fact that Massachusetts contains over 2.1 million houses built before 1978. Two thirds of these inspections visit a house for the first time. These figures have decreased over time, but remarkably, over the majority of first inspections find some lead hazard violations. In line with the trend in inspections, the number of certified lead contractors has also decreased over time. Nonetheless, licensed contractors seem to respond more to the funds available for training than to the changes in the housing stock, as emphasized by the spike starting in 2010, when the American Recovery and Reinvestment Act (ARRA) of 2009 increased local governments' ability to organize training workshops.

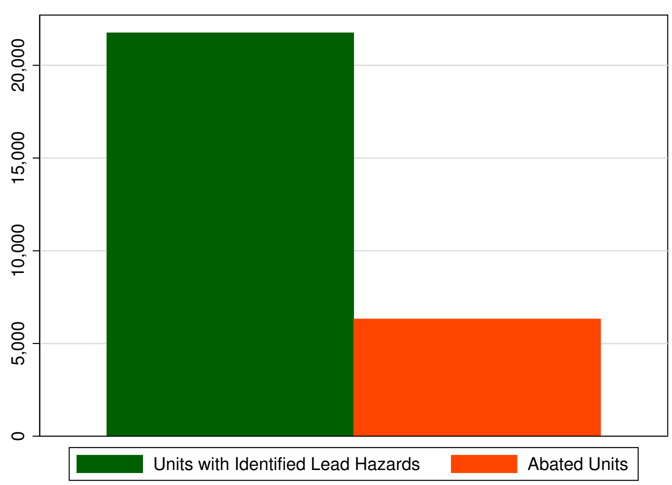
A similar pattern is visible in Ohio, as shown in Figure A.4: after an initial spike in the number of licensed inspectors in 2006, their number goes back to the pre-mandate level, fluctuating between 700 and 800 active licenses per year. In Ohio, the number of licensed contractors does not respond to regulation, and it increases markedly in 2009, similarly to what happens in Massachusetts (Figure A.1). Data on abatement projects in Ohio shows that there have been an average of over 1,000 projects in the fiscal years 2009-2013.

Figure A.1: Enforcement, MA



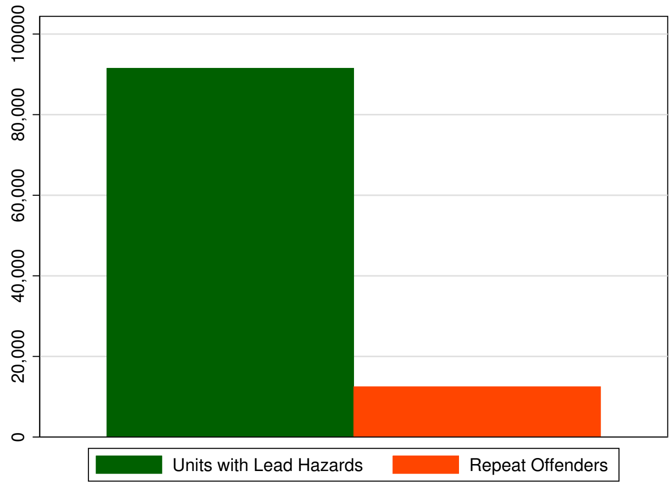
Source: Inspections data from Massachusetts Department of Public Health; lead-licensed contractors from Massachusetts Department of Labor Standards. The figure plots the number of inspections (black dashed), first inspections to a house (green dotted) and first inspections that find violations (red solid) on the left axis, and the number of contractors that are licensed for lead projects (blue dash-dot) on the right axis over calendar time in years.

Figure A.2: Houses with Identified Lead Hazards and Abated Houses, MA



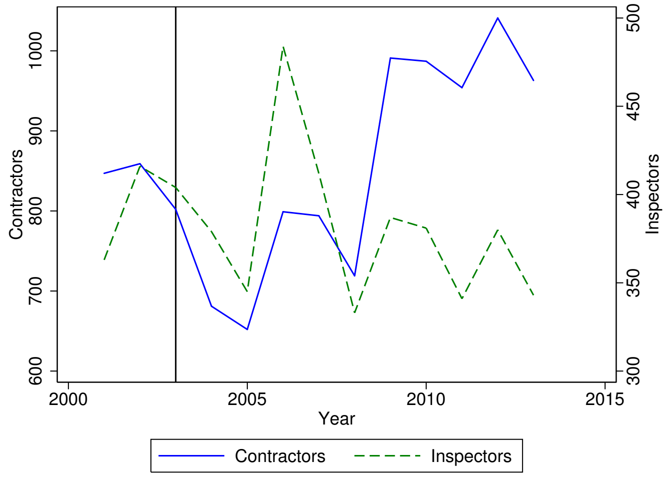
Source: Massachusetts Department of Public Health. The figure plots the number of houses with identified lead hazards (green bar) in Massachusetts for the years 1995-2015. A lead hazard can be identified either by an inspection outcome recording a violation or by an elevated blood lead level. The red bar illustrates how many of these houses are eventually abated.

Figure A.3: Houses with Identified Lead Hazards and Repeat Offender Houses



Sources: Massachusetts Department of Public Health (1995-2015), Maryland Department of the Environment (1995-2015), New Jersey Department of Health (1973-2015), North Carolina Department of Health and Human Services (1993-2015). The figure plots the number of houses with identified lead hazards (green bar) in Massachusetts, Maryland, New Jersey, and North Carolina. A lead hazard can be identified either by an inspection outcome recording a violation or by an elevated blood lead level. The red bar illustrates how many of these houses present a new lead hazard after the first one.

Figure A.4: Enforcement, OH



Source: Ohio Department of Health. The figure plots the number of contractors that are licensed for lead projects (blue solid) on the left axis and the number of lead inspectors on the right axis (green dashed) over calendar time in years. The vertical line indicates the year Ohio introduced a lead abatement mandate, 2003.

B Data Appendix

From the transaction file, I drop properties with missing characteristics and transactions that are not arms-length transfers, such as transactions between family members, to ensure that the sale price reflects the true value of the house.[29] When, according to the assessor file, properties undergo major renovations, I replace construction year with the renovation year because these renovations likely change the lead status of the house and because the renovations are public information available to the buyers. Indeed, in these cases, the assessor deems the original construction year not informative of the value of the house. My results are robust to both dropping these properties and including them with their original vintage. Then, I assign each geocoded property to a census tract according to 2010 boundaries, dropping observations in areas that were not tracted in 1980. To avoid comparing houses in neighborhoods that are fundamentally different in terms of age of the housing stock, I drop all tracts with only new or only old houses. This leaves over 27 million transactions for 22 million properties in 44,170 census tracts. Furthermore, in my preferred specification, I limit the sample for implementing states to observations in a window of $[-6, 10]$ years around the introduction of the policies to obtain a more balanced panel. Columns 1-2 in Appendix Table C.5 show that neither this sample restriction nor the unbalanced nature of the full panel affect the results.

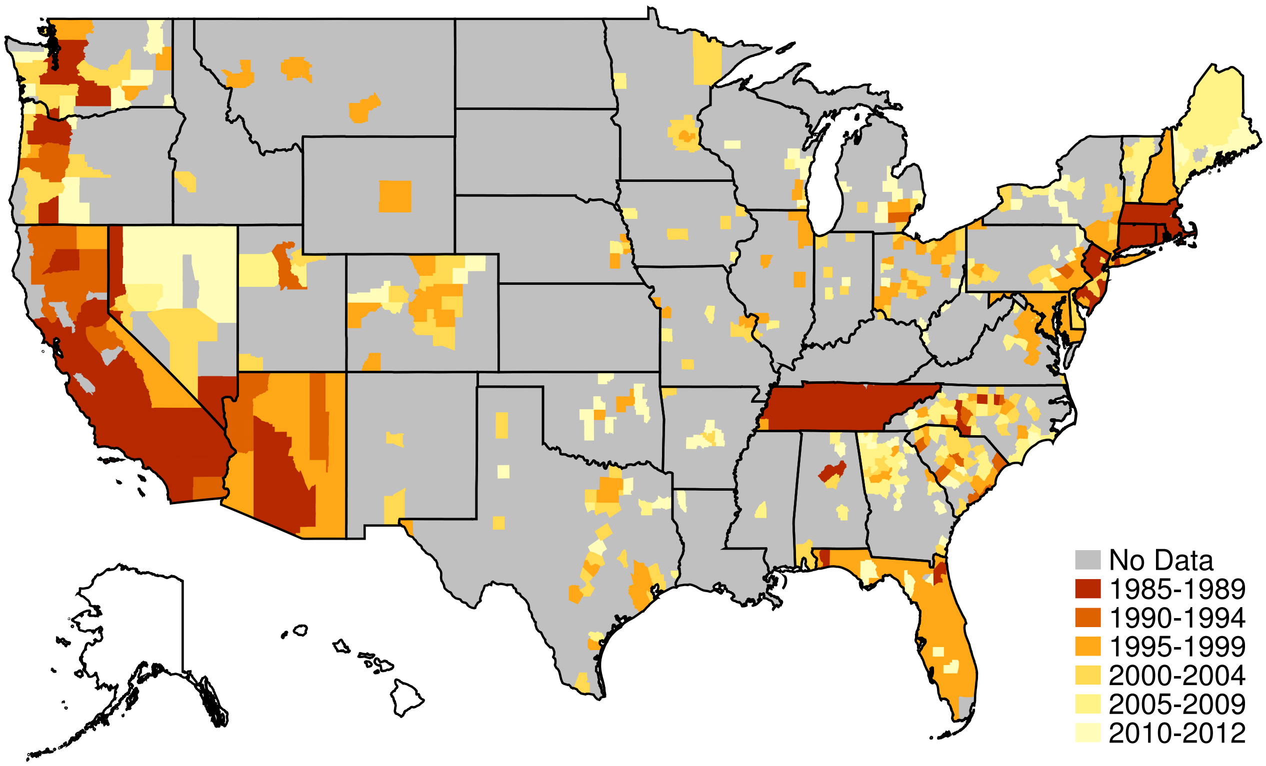
In the empirical analysis I estimate the effect of the mandates on house values separately for single- and multi-family homes. I include condominiums among the multi-family houses, as condominium conversion is as much an endogenous choice as tenancy is. Moreover, in my sample, 57 percent of multi-family properties and 40 percent of condominiums are rented, while only 21 percent of single-family properties are rented.

Table B.1 displays the characteristics of the housing stock in my two housing datasets: the DataQuick data repository and the AHS, as well as selected demographic characteristics from the

[29]Specifically, I drop duplicate transactions, transactions for less than $10,000, not arms-length transfer, group-property sales, subdivisions and property splits, transactions that include liens or encumbrances or only partial interest in the property, and repeat sales. Moreover, I drop properties where any of the following characteristics is missing: address, square footage, year of construction.

AHS. Although the DataQuick and AHS samples are similar in terms of the size of the housing units, as well as house values and age of the housing stock, houses in the DataQuick sample are somewhat newer, and their average price per square foot is higher, likely reflecting selection in terms of what houses are transacted.

Figure B.1: Transaction Data Coverage



The figure shows a heat map of the coverage of the DataQuick data repository, by county and initial coverage year. Darker shades indicate counties that have been in the database the longest.

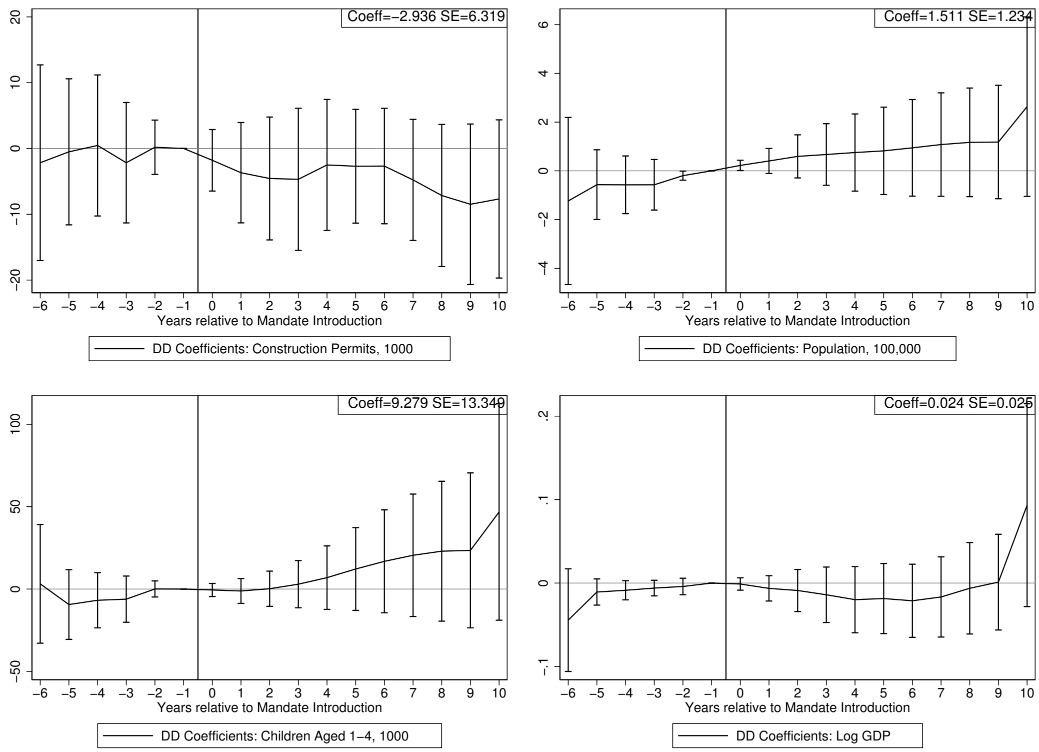
Table B.1: Summary Statistics

Sample	DataQuick		AHS
Housing Structure	Multi-Family	Single-Family	All
	(1)	(2)	(3)
Price per Square Foot	167.85	140.02	122.88
(Assessed Value in AHS)	(610.73)	(922.60)	(263.71)
Vintage	1973.15	1967.90	1953.89
	(22.61)	(24.53)	(22.62)
Square Footage	1475.78	1695.60	1631.30
	(893.75)	(721.78)	(1299.59)
Number of Rooms	2.34	3.67	5.40
	(4.08)	(5.10)	(1.89)
HH Has Child <6			0.15
			(0.36)
HH Has Child 6-11			0.15
			(0.36)
House is Rented			0.40
			(0.49)
Monthly Rent			629.96
			(440.89)
Observations	3,607,422	15,068,454	211,994

Notes: The table reports summary statistics from the DataQuick sample (years 1988-2012) by multi- and single family, and the AHS sample (years 1985-2011). In the AHS sample, the price variable is the assessed value of home-owned houses and vintage is a 10-year bin starting in 1900 (AHS). Standard deviations are shown in parentheses.

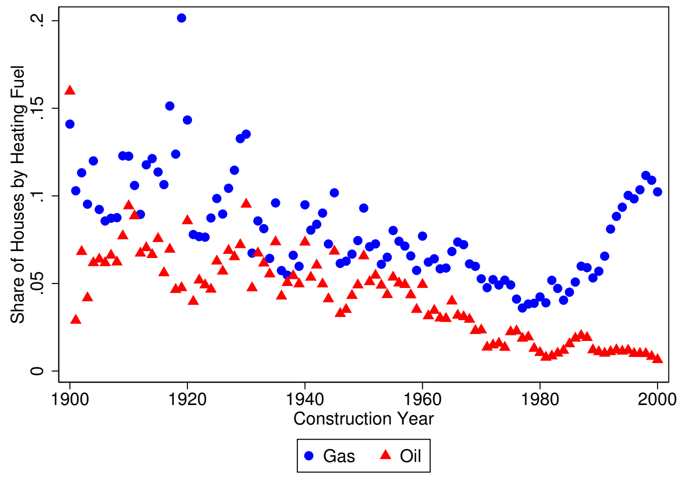
C Additional Figures and Tables

Figure C.1: State-level Trends in Covariates



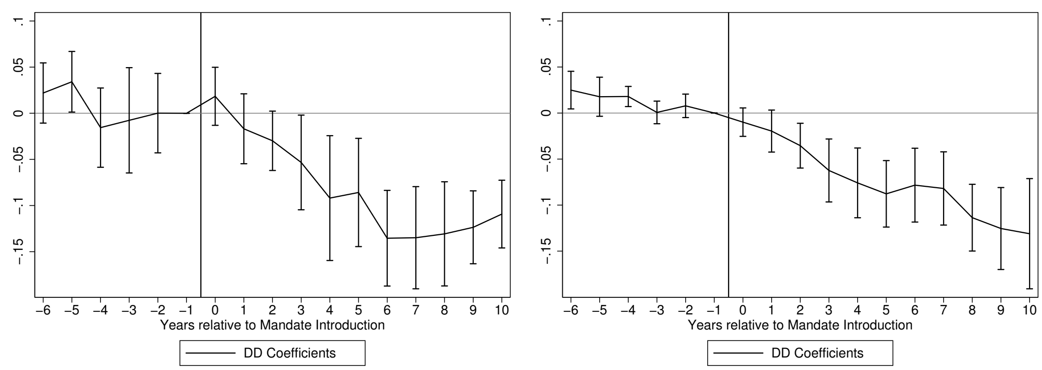
Notes: Construction Permits data were retrieved from the Census Bureau, Building Permits Survey; Population data are provided by the National Cancer Institute, SEER Program; State GDP data were retrieved from the Bureau of Economic Analysis. The figures plot DD coefficients on year-by-year mandate dummies and reports coefficient and standard error for a single post-mandate indicator in the top-right box. Outcome variables are indicated in each plot. The vertical line indicates the introduction of the mandate. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. State, year, and trends for treated states are included. T-1 is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level.

Figure C.2: Share of Houses Built, by Heating Fuel



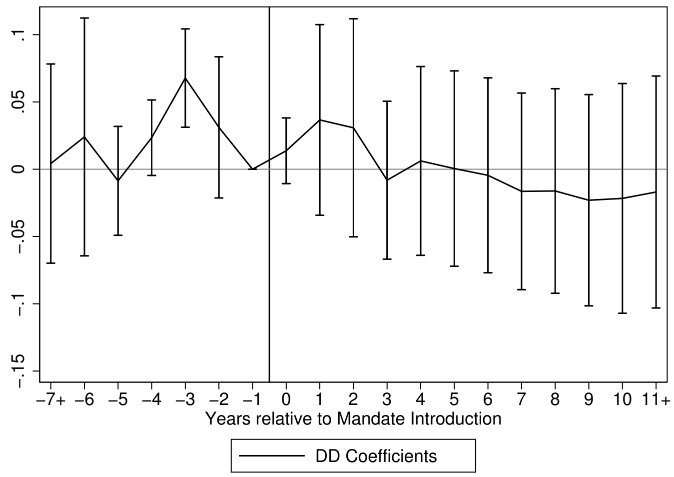
The figure plots the share of houses heated by gas (blue dots) and oil (red triangles) built between the year 1900 and the year 2000 in the DataQuick sample.

Figure C.3: Price Effects, Balanced Panel



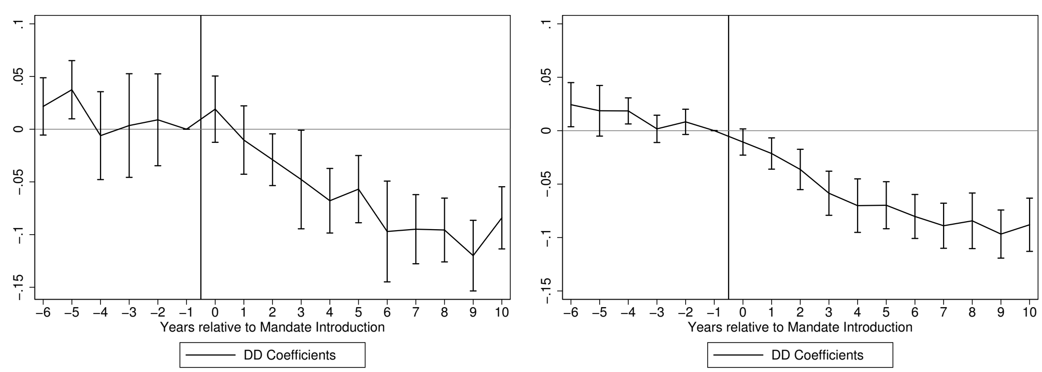
Notes: The figure plots DD estimates on the transaction sample for multi- (left panel) and single-family buildings (right panel) from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. The sample is limited to a $[-6, 10]$ window around introduction of the mandate in treated states. Only treated states covered from one year prior to seven years after the mandates and nontreated states covered since 1988 are included. Tract-year, tract-vintage and vintage-year fixed effects are included. Standard errors clustered at the state level are shown in parentheses. The vertical line at $t = 0$ indicates the introduction of the mandate in each state.

Figure C.4: Price Effects, Placebo Specification in Nontreated States



Notes: The figure plots DD estimates on the transaction sample for multi-family buildings in nontreated states from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included. Standard errors clustered at the state level are shown in parentheses. The vertical line at $t = 0$ indicates the introduction of the placebo mandate in each state. Placebo mandates are randomly assigned to nontreated states. Each placebo mandate is introduced in a year when an actual mandate is introduced in a treated state.

Figure C.5: Price Effects, Dropping 2008-onward



Notes: The figure plots DD coefficients on year-by-year mandate dummies, estimated on the DataQuick samples (1988-2007) of multi- (left panel) and single-family (right panel) houses. Each census tract is weighted by 1980 population. The outcome variable is the logarithm of the price per square foot. The vertical line indicates the introduction of the mandate. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Tract-year, tract-vintage and vintage-year fixed effects are included. T-1 is the omitted category. The vertical bars are 95 percent confidence intervals. Standard errors are clustered at the state level (42 clusters).

Table C.1: Sale Effects

Dependent Variable	Probability of Sale (X 1,000				Number of Sales	
Sample	Multi-Family		Single-Family		Multi-Family	Single-Family
	(1)	(2)	(3)	(4)	(5)	(6)
Mandate Effects on Old Houses						
After Mandate	-0.984		-0.502		-0.209	0.041
	(0.512)		(0.447)		(0.313)	(0.296)
0-3 Years after Mandate		-1.385		-0.194		
		(0.518)		(0.368)		
4-6 Years after Mandate		-1.315		-0.623		
		(0.507)		(0.499)		
7-10 Years after Mandate		0.028		-0.887		
		(0.660)		(0.697)		
N	552,414	552,414	1,945,369	1,945,369	552,414	1,945,369
Outcome Mean, New Homes	7.165	7.165	11.043	11.043	4.366	6.071
Outcome Mean, Old Homes	6.683	6.683	11.635	11.635	3.438	6.575

Notes: The table presents DD estimates on the transaction sample of multi- (Columns 1-2, 5) and single-family properties (Column 3-4, 6) from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. Each observation is a tract-year-vintage cell, and the dependent variable is the share of houses in the cell transacted in that year multiplied by 1,000 (Columns 1-4) or the Number of Transactions (Columns 5-6). Tract-year, tract-vintage and vintage-year fixed effects arse included. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. The mean of the outcome variable implementing states before the mandates is shown separetely for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.2: Sales Compositon Effects

Dependent Variable	Number of Rooms (1)	Room Size (2)	Square Footage (3)	Renovations (4)	Conditions (5)
Panel A: Mandate Effects on Old Houses, Multi-Family Properties					
0-3 Years after Mandate	-0.001	-0.564	13.530	0.000	0.045
	(0.014)	(2.769)	(13.506)	(0.005)	(0.022)
4-6 Years after Mandate	0.055	2.720	12.006	0.009	-0.058
	(0.035)	(3.150)	(31.184)	(0.007)	(0.054)
7-10 Years after Mandate	0.040	-1.436	7.867	0.006	0.057
	(0.024)	(2.998)	(35.718)	(0.006)	(0.046)
N	1,224,194	1,478,477	1,478,477	1,478,477	1,478,466
Mean Outcome, New Homes	5.202	274.871	1482.027	0.089	2.662
Mean Outcome, Old Homes	4.962	237.626	1837.011	0.206	2.792
Panel B: Mandate Effects on Old Houses, Single-Family Properties					
0-3 Years after Mandate	0.018	-54.179	-414.4467	0.001	0.003
	(0.013)	(60.159)	(441.190)	(0.003)	(0.006)
4-6 Years after Mandate	0.018	78.028	377.899	-0.001	-0.040
	(0.009)	(97.383)	(483.808)	(0.003)	(0.029)
7-10 Years after Mandate	0.027	21.688	66.650	-0.005	0.011
	(0.010)	(79.799)	(378.062)	(0.004)	(0.030)
N	7,867,030	8,329,855	8,329,855	8,329,855	8,329,839
Mean Outcome, New Homes	7.045	291.322	2067.409	0.150	2.654
Mean Outcome, Old Homes	6.049	241.588	1492.137	0.346	2.513

Notes: The table presents DD estimates on the transaction sample of multi- (Panel A) and single-family properties (Panel B) from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The dependent variable is indicated in each column. Tract-year, tract-vintage and vintage-year fixed effects arse included. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. The mean of the dependent variable in implementing states before the mandates is shown separetely for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.3: Price Effects, by Occupancy

Dependent Variable	Log Price per Square Foot			
Sample	Rental Properties		Owner-Occupied Properties	
	(1)	(2)	(3)	(4)
Mandate Effects on Old Houses				
After Mandate	-0.099		-0.054	
	(0.016)		(0.010)	
0-3 Years after Mandate		-0.056		-0.026
		(0.014)		(0.011)
4-6 Years after Mandate		-0.117		-0.074
		(0.019)		(0.015)
7-10 Years after Mandate		-0.147		-0.089
		(0.020)		(0.012)
N	5,575,504	5,575,504	14,628,577	14,628,577
Price per SqFt, New Homes	106.037	106.037	108.901	108.901
Price per SqFt, Old Homes	90.337	90.337	100.990	100.990

Notes: The table presents DD estimates on the transaction sample of rental (Columns 1-2) and owner-occupied houses (Columns 3-4) from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included. For implementing states, the sample is limited to a $[-6, 10]$ window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.4: Price Effects, 5- and 10-Year Windows around Mandates

Dependent Variable	Log Price per Square Foot			
Sample	1973-1983 Vintages		1968-1988 Vintages	
	(1)	(2)	(3)	(4)
Mandate Effects on Old Houses				
0-3 Years after Mandate	0.008	-0.006	-0.015	-0.020
	(0.011)	(0.004)	(0.015)	(0.005)
4-6 Years after Mandate	-0.024	-0.017	-0.061	-0.042
	(0.017)	(0.006)	(0.021)	(0.012)
7-10 Years after Mandate	-0.025	-0.015	-0.081	-0.055
	(0.013)	(0.005)	(0.015)	(0.011)
N	4,205,906	4,174,389	7,689,644	7,689,644
Price per SqFt, New Homes	98.16	98.15	108.98	108.98
Price per SqFt, Old Homes	114.70	115.06	112.26	112.26
State-Year FE	X		X	
Vintage-Year FE	X		X	
State-Vintage FE	X		X	
Tract-Year FE		X		X
Tract-Vintage FE		X		X

Notes: The table presents DD estimates on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The sample is limited to houses built between 1973 and 1983 in Columns 1-2 and between 1968 and 1988 in Columns 3-4. The outcome variable is the logarithm of the transaction price divided by square footage of the house. Tract-year, tract-vintage and vintage-year fixed effects are included, where vintage is construction year. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (42 clusters) are shown in parentheses.

Table C.5: Price Effects for Multi-Family Houses, Alternative Specifications

Dependent Variable	Log Price per Square Foot					Log Price
Sample	Full Sample (1)	Balanced Panel (2)	Implementing States Only (3)	No Weights (4)	AHS States (5)	Main Sample (6)
Panel A: Mandate Effects on Old Houses, Multi-Family Properties						
0-3 Years after Mandate	-0.032	-0.021	-0.037	-0.027	-0.024	-0.025
	(0.009)	(0.014)	(0.018)	(0.016)	(0.014)	(0.013)
4-6 Years after Mandate	-0.101	-0.104	-0.059	-0.103	-0.097	-0.109
	(0.023)	(0.031)	(0.032)	(0.032)	(0.029)	(0.022)
7-10 Years after Mandate	-0.117	-0.124	-0.052	-0.118	-0.126	-0.126
	(0.015)	(0.017)	(0.037)	(0.021)	(0.018)	(0.019)
11+ Years after Mandate	-0.116					
	(0.020)					
N	3,961,195	3,552,256	204,525	3,669,173	3,583,352	3,607, 422
Price per SqFt, New Homes	103.866	105.763	105.763	105.747	105.763	105.763
Price per SqFt, Old Homes	74.213	73.380	77.373	77.366	77.380	77.373
Panel B: Mandate Effects on Old Houses, Single-Family Properties						
0-3 Years after Mandate	-0.043	-0.041	-0.019	-0.046	-0.042	-0.047
	(0.012)	(0.013)	(0.010)	(0.014)	(0.013)	(0.012)
4-6 Years after Mandate	-0.088	-0.089	-0.009	-0.089	-0.093	-0.094
	(0.018)	(0.019)	(0.015)	(0.021)	(0.018)	(0.017)
7-10 Years after Mandate	-0.101	-0.114	0.005	-0.104	-0.107	-0.114
	(0.016)	(0.018)	(0.016)	(0.018)	(0.016)	(0.017)
11+ Years after Mandate	-0.103					
	(0.020)					
N	17,047,218	14,698,354	1,512,625	15,980,520	14,773,465	15,068,454
Price per SqFt, New Homes	107.922	108.628	108.628	108.458	109.898	108.628
Price per SqFt, Old Homes	101.172	102.204	102.204	102.134	102.466	102.204

Notes: The table presents DD estimates on DataQuick samples (1988-2012) of multi- (Panel A) and single-family (Panel B) houses. Observations are weighted by 1980 population in tract. The outcome variable is log of price per sqft, but for Column 6 which uses log price as outcome and controls for a quadratic in square footage. Tract-year, tract-vintage and vintage-year FEs are included. For implementing states, the sample includes a $[-6, 10]$ window around the introduction of the mandates, but for Column 1, which includes the full sample. Column 2 presents estimates from a balanced sample that includes all non-implementing states and only those implementing states with observations both before and after the mandate (CT, GA, MI, NC, OH, RI). Column 3 limits the sample to implementing states only (CT, GA, IL, MD, MI, MN, MO, NH, NC, OH, RI, VT). Column 4 removes the 1980 tract population weights. Column 5 limits the sample to states included also in the AHS sample (AL, AZ, AR, CA, CO, CT, HI, IL, IN, IA, MD, MI, NV, NM, NY, OH, OK, OR, PA, RI, TN, TX, UT, VA, VT, WI). Each column shows average price per sqft of new and old houses in implementing states before the mandates. Standard errors clustered at state level (42 clusters) are in parentheses, but for Column 3 which clusters SEs at state-vintage level.

Table C.6: Price Effects, Assessed Value in AHS Sample

Dependent Variable	Log Assessed Price per Square Foot			
Specification	Baseline (1)	State-Vintage Trends (2)	Property FE (3)	DQ Sample (4)
Mandate Effects on Old Houses				
0-3 Years after Mandate	-0.004	-0.036	0.013	-0.027
	(0.028)	(0.014)	(0.021)	(0.018)
4-6 Years after Mandate	-0.017	-0.062	-0.034	-0.039
	(0.030)	(0.041)	(0.026)	(0.036)
7-10 Years after Mandate	-0.028	-0.079	-0.056	-0.073
	(0.043)	(0.029)	(0.031)	(0.030)
11+ Years after Mandate	-0.027	-0.054	-0.047	
	(0.035)	(0.063)	(0.047)	
N	115,919	115,919	114,722	89,004
Value per SqFt, New Homes	73.375	73.375	73.397	88.934
Value per SqFt, Old Homes	54.056	54.056	53.818	67.967
State-Year FE	X	X	X	X
Vintage-Year FE	X	X	X	X
State-Vintage FE	X	X	X	X
State-Vintage Trends		X	X	X
Property FE			X	

Notes: The table presents DD estimates on the AHS sample for the years 1985-2011. The outcome variable is the logarithm of the unit assessed value divided by square footage of the unit. The set of fixed effects included in each specification is defined in each column. Average value per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column for each estimation sample. Standard errors clustered at state level (36 clusters) are shown in parentheses.

Table C.7: Price Effects by Year of Construction

Dependent Variable	Log Price per Square Foot		
Cohort	1800-1949	1950-1977	1990s
	(1)	(2)	(3)
Mandate Effects on Cohort			
0-3 Years after Mandate	-0.009	-0.019	0.016
	(0.012)	(0.005)	(0.012)
4-6 Years after Mandate	-0.080	-0.044	0.040
	(0.026)	(0.007)	(0.021)
7-10 Years after Mandate	-0.074	-0.050	0.057
	(0.029)	(0.011)	(0.017)
N		18,891,208	
Price per SqFt, 1980s		108.37	
Price per SqFt, Cohort	88.40	104.93	108.19

Notes: The table presents DD estimates from a single regression on the transaction sample from DataQuick for the years 1988-2012, where each observation is weighted by tract population in 1980. The oldest (1700s) and most recent (2000s) vintages are dropped from the sample since there are too few observations in these categories. The table presents DD coefficients on period-by-period indicators for homes built 1800-1949, 1950-1977, and in the 1990s relative to homes built 1978-1989. Tract-year, tract-vintage and old-year fixed effects are included. For implementing states, the sample is limited to a [-6,10] window around the introduction of the mandates. Average price per square foot in implementing states before the mandates is shown separately for each vintage at the bottom of each column. Standard errors clustered at the state level (38 clusters) are shown in parentheses.

Table C.8: Price Effects, by State

Dependent Variable	Log Price per Square Foot			
State	Connecticut	Michigan	Ohio	Rhode Island
	(1)	(2)	(3)	(4)
Panel A: Mandate Effects on Old Houses, Multi-Family Properties				
0-3 Years after Mandate	-0.043	-0.021	0.040	0.085
	(0.025)	(0.019)	(0.013)	(0.022)
4-6 Years after Mandate	-0.070	-0.257	-0.042	0.086
	(0.032)	(0.040)	(0.023)	(0.026)
7-10 Years after Mandate	-0.027	-0.283	-0.122	-0.072
	(0.029)	(0.041)	(0.028)	(0.028)
N	50,287	17,798	47,359	32,093
Price per SqFt, New Homes	123.533	137.862	87.803	84.755
Price per SqFt, Old Homes	90.762	143.595	61.514	51.260
Panel B: Mandate Effects on Old Houses, Single-Family Properties				
0-3 Years after Mandate	-0.016	-0.050	0.024	0.053
	(0.009)	(0.012)	(0.004)	(0.009)
4-6 Years after Mandate	-0.030	-0.214	-0.054	0.048
	(0.008)	(0.016)	(0.006)	(0.011)
7-10 Years after Mandate	-0.023	-0.251	-0.125	-0.022
	(0.008)	(0.021)	(0.007)	(0.016)
N	198,666	200,085	518,416	80,713
Price per SqFt, New Homes	116.989	144.838	92.059	111.062
Price per SqFt, Old Homes	116.518	141.739	82.981	93.941

Notes: The table presents DD estimates on DataQuick samples (1988-2012) of multi- (Panel A) and single-family (Panel B) houses. Observations are weighted by 1980 population in tract. The outcome variable is the logarithm of the price per square foot. Each column is estimated on the state indicated at the top, limiting the sample to a $[-5,7]$ window around the introduction of the mandate in that state, if available. Each regression includes tract-year and tract-vintage fixed effects. Average price per square foot in each state before the mandate is shown separately for new and old houses at the bottom of each column. Standard errors clustered at the tract level are shown in parentheses.

Table C.9: Price Effects for Multi-Family Houses, Alternative Sets of Fixed Effects

Dependent Variable	Log Price per Square Foot			
Specification	State-Year & State-Vintage FE	County-Year & County-Vintage FE	Tract-Year & State-Vintage FE	State-Year FE State-Vintage Trends
	(1)	(2)	(3)	(4)
Panel A: Mandate Effects on Old Houses, Multi-Family Properties				
0-3 Years after Mandate	-0.055	-0.056	-0.025	-0.020
	(0.032)	(0.028)	(0.017)	(0.052)
4-6 Years after Mandate	-0.209	-0.189	-0.094	-0.140
	(0.045)	(0.044)	(0.034)	(0.086)
7-10 Years after Mandate	-0.187	-0.165	-0.115	-0.134
	(0.035)	(0.025)	(0.023)	(0.078)
N	3,646,188	3,645,874	3,621,778	3,607,422
Price per SqFt, New Homes	105.763	105.763	105.763	105.763
Price per SqFt, Old Homes	77.373	77.373	77.373	77.373
Panel B: Mandate Effects on Old Houses, Single-Family Properties				
0-3 Years after Mandate	-0.078	-0.069	-0.048	0.018
	(0.028)	(0.020)	(0.013)	(0.017)
4-6 Years after Mandate	-0.181	-0.146	-0.089	-0.009
	(0.054)	(0.031)	(0.020)	(0.040)
7-10 Years after Mandate	-0.248	-0.174	-0.116	-0.026
	(0.050)	(0.028)	(0.018)	(0.034)
N	15,086,525	15,086,374	15,082,650	15,068,454
Price per SqFt, New Homes	108.628	108.628	108.628	108.628
Price per SqFt, Old Homes	102.204	102.204	102.204	102.204
State-Vintage FE	X		X	X
County-Vintage FE		X		
State-Vintage Trends				X

Notes: The table presents DD estimates on on DataQuick samples (1988-2012) of multi- (Panel A) and single-family (Panel B) houses. Observations are weighted by 1980 population in tract. The outcome variable is the logarithm of the transaction price divided by square footage of the house. The set of fixed effects included in each specification is defined in each column. Average price per square foot in implementing states before the mandates is shown separately for new and old houses at the bottom of each column for each estimation sample. Standard errors clustered at state level (42 clusters) are shown in parentheses.

Table C.10: Allocation Summary Statistics

Sample	Old Houses		New Houses	
Period	Before Mandate	After Mandate	Before Mandate	After Mandate
	(1)	(2)	(3)	(4)
HH Has Child <6	0.17	0.15	0.15	0.13
	(0.37)	(0.36)	(0.36)	(0.34)
HH Has Child 6-11	0.16	0.16	0.15	0.14
	(0.37)	(0.37)	(0.36)	(0.35)
HH Has Member >59	0.22	0.23	0.28	0.29
	(0.42)	(0.42)	(0.45)	(0.45)
Log Income	10.60	10.75	10.30	10.46
	(1.02)	(1.14)	(1.09)	(1.15)
College Educated	0.056	0.65	0.45	0.53
	(0.50)	(0.48)	(0.50)	(0.50)
Black HH Head	0.10	0.16	0.14	0.17
	(0.31)	(0.37)	(0.35)	(0.37)
Observations	33,765	7,879	133,779	36,571

Notes: The table reports summary statistics of characteristics of households living in old and new houses before and after a mandate, from the AHS sample (years 1985-2011). Standard deviations are shown in parentheses.

Table C.11: Effects on Probability of Moving

Dependent Variable	Change in Residents	
Sample	Multi-Family	Single-Family
	(1)	(2)
Panel A: Mandate Effects on Old Houses, Single Post-Period		
After Mandate	0.013	-0.003
	(0.018)	(0.019)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods		
0-3 Years after Mandate	0.057	0.000
	(0.015)	(0.027)
4-6 Years after Mandate	-0.065	0.001
	(0.034)	(0.018)
4-6 Years after Mandate	0.014	-0.015
	(0.028)	(0.032)
10+ Years after Mandate	0.041	-0.016
	(0.019)	(0.028)
N	48,826	104,390
Outcome Mean, New Homes	0.387	0.139
Outcome Mean, Old Homes	0.340	0.118

Notes: The table presents DD estimates on the AHS sample of multi- (Column 1) and single-family houses (Column 2) for the years 1985-2011. The outcome variable is an indicator for changes in residents in a given house. State-year, year-vintage, month of interview and house fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table C.12: Allocation Effects, Robustness Checks

Dependent Variable	HH Has Child <6	HH Has Child <6	Youngest in HH >59	Youngest in HH >59
	(1)	(2)	(3)	(4)
Panel A: Mandate Effects on Old Houses, Single Post-Period				
After Mandate	-0.020	-0.045	0.030	-0.044
	(0.009)	(0.012)	(0.017)	(0.020)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods				
0-3 Years after Mandate	-0.036	-0.052	0.013	-0.036
	(0.012)	(0.012)	(0.022)	(0.018)
4-6 Years after Mandate	-0.028	-0.044	0.026	-0.036)
	(0.017)	(0.023)	(0.017)	(0.020)
4-6 Years after Mandate	0.005	-0.025	0.019	-0.064
	(0.017)	(0.023)	(0.022)	(0.029)
10+ Years after Mandate	0.002	-0.054	0.088	-0.013
	(0.015)	(0.028)	(0.018)	(0.036)
N	211,994	211,994	211,994	211,994
Outcome Mean, New Homes	0.162	0.162	0.199	0.199
Outcome Mean, Old Homes	0.150	0.150	0.266	0.266
State-Year FE	X	X	X	X
Vintage-Year FE	X	X	X	X
State-Vintage FE	X		X	
State-Vintage Trends		X		X
Property FE		X		X

Notes: The table presents DD estimates on the AHS sample for the years 1985-2011. The outcome variables are defined in each column. State-year, year-vintage, state-vintage and month of interview fixed effects are included in Columns 1 and 3; state-year, year-vintage, month of interview, house fixed effects and state-vintage-specific linear trends are included in Columns 2 and 4. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table C.13: Allocation Effects by Housing Structure

Sample	Multi-Family			Single-Family		
Dependent Variable	HH Has Child <6	HH Has Child 6-11	Youngest in HH >59	HH Has Child <6	HH Has Child 6-11	Youngest in HH >59
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
After Mandate	-0.052	-0.041	-0.029	-0.021	-0.023	0.042
	(0.032)	(0.022)	(0.045)	(0.017)	(0.027)	(0.017)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods						
0-3 Years after Mandate	-0.084	-0.006	-0.042	-0.035	0.010	0.030
	(0.022)	(0.013)	(0.042)	(0.019)	(0.024)	(0.016)
4-6 Years after Mandate	-0.049	-0.087	-0.008	-0.021)	-0.042	0.028
	(0.045)	(0.033)	(0.064)	(0.029)	(0.028)	(0.011)
7-10 Years after Mandate	-0.015	-0.062	-0.051	-0.011	-0.033	0.040
	(0.036)	(0.027)	(0.076)	(0.031)	(0.039)	(0.024)
10+ Years after Mandate	-0.032	-0.025	-0.012	0.006	-0.057	0.105
	(0.039)	(0.029)	(0.036)	(0.028)	(0.055)	(0.027)
N	53,581	53,581	53,581	114,990	114,990	114,990
Outcome Mean, New Homes	0.059	0.037	0.376	0.214	0.233	0.124
Outcome Mean, Old Homes	0.130	0.107	0.314	0.154	0.172	0.247

Notes: The table presents DD estimates on the AHS sample of multi- (Columns 1-3) and single-family houses (Columns 4-6) for the years 1985-2011. Outcome variables are defined in each column. State-year, year-vintage, month of interview and unit fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.

Table C.14: Rental Market Effects, Extensive and Intensive Margins

Sample	Multi-Family			Single-Family		
Dependent Variable	Rental Entry	Rental Exit	Log Monthly Rent	Rental Entry	Rental Exit	Log Monthly Rent
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mandate Effects on Old Houses, Single Post-Period						
After Mandate	0.144	0.013	0.022	-0.011	0.074	0.260
	(0.092)	(0.010)	(0.039)	(0.011)	(0.053)	(0.031)
Panel B: Mandate Effects on Old Houses, Multiple Post-Periods						
0-3 Years after Mandate	0.113	0.014	0.039	-0.007	0.033	0.359
	(0.078)	(0.016)	(0.032)	(0.008)	(0.039)	(0.072)
4-6 Years after Mandate	0.170	0.019	0.111	-0.013)	0.097	0.227
	(0.093)	(0.011)	(0.081)	(0.015)	(0.088)	(0.040)
7-10 Years after Mandate	0.222	0.015	-0.131	-0.015	0.080	0.088
	(0.121)	(0.007)	(0.091)	(0.011)	(0.113)	(0.125)
10+ Years after Mandate	0.110	-0.003	0.011	-0.013	0.092	0.250
	(0.108)	(0.009)	(0.078)	(0.014)	(0.089)	(0.073)
N	4,244	31,533	44,860	83,480	8,808	12,551
Outcome Mean, New Homes	0.110	0.017	6.150	0.011	0.110	6.108
Outcome Mean, Old Homes	0.069	0.013	5.785	0.020	0.155	5.830

Notes: The table presents DD estimates on the AHS sample of multi- (Columns 1-3) and single-family houses (Columns 4-6) for the years 1985-2011. Outcome variables are defined in each column. State-year, year-vintage, month of interview and house fixed effects are included. Mean outcome values in implementing states before the mandates are shown separately for new and old houses at the bottom of each column. Standard errors clustered at the state level (36 clusters) are shown in parentheses.